

THE THEORY OF
MONETARY POLICY

THE THEORY OF MONETARY POLICY

WITH SPECIAL REFERENCE TO THE
RELATION BETWEEN INTEREST RATES
AND PRICES

BY

BHALCHANDRA P. ADARKAR

ADAM SMITH PRIZEMAN, CAMBRIDGE UNIVERSITY

PROFESSOR OF ECONOMICS AT THE

BENARES HINDU UNIVERSITY

LONDON

P. S. KING & SON, LTD.

ORCHARD HOUSE, 14 GREAT SMITH STREET

WESTMINSTER, S.W.1

1935

PRINTED IN GREAT BRITAIN

TO THE SACRED MEMORY
OF
MY FATHER

PREFACE

IN modern monetary philosophy no subject has perhaps provided more food for thought or more cause for disagreement than the use of the bank-rate mechanism towards the control of prices and of the courses of production and employment. There are, as even the most casual reader must find for himself, not only the keenest differences as to the ultimate ends of monetary policy, but the most fundamental divergences of views as regards the technique of monetary control, the precise manner of operation of the various instruments at the disposal of banking systems and as regards the interpretation of facts and figures relating to the mutual interactions of monetary events and industrial developments. It would be audacious on the part of anyone, who had not the necessary practical experience born of a constant and long-standing intellectual contact with current economic developments, to pretend to make a first-rate and original contribution to thought in this highly controversial field. This small book, however, is not intended to be a comprehensive treatise on all aspects of monetary policy ; nor is it written with any ambition of throwing a searchlight on the vast statistical and theoretical material made available by contemporary events on the one hand and by their commentators on the other. My object here is only to bring together some modest *obiter dicta* on banking policy, on the relation between interest rates and prices and on the ultimate ends of monetary control and to give them a coherent shape. Not all of us economists can profess to have that mastery over mind and matter that is essential to the production of full-fledged text-books. At the same time, I believe, a useful purpose will be served

by the presentation of such theoretical schemata by those who are engaged in the study of kindred subjects, so that by exchange of ideas and mutual criticism a system of doctrines may be built up in which the differences shall be less emphasised than the agreements.

On a perusal of the book it may appear that I have devoted considerable space to current controversies on the subject of bank-rate policy. This is partly due to the fact that I began working on these problems at a time when, on the publication of Keynes's *Treatise on Money*, these controversies were at their keenest and the issues seemed to get more and more clear-cut like refined diamonds as the discussions advanced. But more particularly, it is due to my belief that in any discussion of an "ideal" rate of interest, it is impossible not to make reference to the various notions then put forward.

Ricardo¹ was probably the first to speak of a "natural" rate of interest in the modern sense, although he did not develop the notion sufficiently. Since then we have been made acquainted by monetary writers with a variety of other analogous concepts. It will not be difficult for the reader, however, to discern the intellectual genealogy of this effort and he will at once perceive how deeply indebted I am to J. M. Keynes, that leader of modern monetary thought, in much that I have to say in the following pages. In places, of course, I have taken the liberty of differing from him, particularly in the application of the "natural" rate theory to practical banking and as regards the ultimate ends of monetary policy, but the differences are of small consequence. For my attempt is mainly to explore parallel vistas of thought rather than to contradict, and I firmly believe that Keynes has given us a new vision and a new methodology of approach

¹ Cf. *Essays* (Gonner edition), p. 35, where he speaks of a "natural level of interest."

towards the difficult problems of monetary science, which seem to get more, not less, complex with the passage of time. That is only Cæsar's due, however, and it will be realised by the reader that in the following pages my allegiance, where such was necessary, has been eclectic and impartial.

I must here express my gratitude to Mr. D. H. Robertson for kindly replying to some of my queries and to Dr. Brinley Thomas of the London School of Economics for going through the portions dealing with the Wicksellian theory and offering helpful criticism. I need scarcely add, however, that the responsibility for views expressed in the following pages is entirely mine.

I am also particularly thankful to the Editor of the *Economic Journal* for permission to incorporate in this book a Note by me on "Fisher's Real Rate Doctrine," which appeared in the *Journal* (June 1934), and to Messrs. Riefler & Simmons and the American Economic Association for allowing me to reproduce from the *American Economic Review* (June 1933) a Chart showing relative movements of long- and short-term rates.

B. P. ADARKAR.

BENARES HINDU UNIVERSITY,
INDIA.

CONTENTS

CHAP.	PAGE
PREFACE	vii
PRELIMINARY	I
I. THE ACTIVE AND PASSIVE ASPECTS	3
II. FISHER'S REAL RATE	8
III. CASSEL'S TRUE RATE	15
IV. INTEREST AS COST	21
V. WICKSELL'S NORMAL RATE	24
VI. DR. HAYEK'S EQUILIBRIUM RATE	32
VII. MR. SRAFFA'S COMMODITY RATE	41
VIII. KEYNES'S NATURAL RATE	45
IX. THE ENDS OF MONETARY POLICY	53
X. THE PROBLEM OF EXTERNAL EQUILIBRIUM	69
XI. IS BARTER THEORY RELEVANT?	86
XII. THE NATURE OF CAPITAL	92
XIII. THE TRUE CONCEPT OF THE NATURAL RATE	101
XIV. LONG-TERM AND SHORT-TERM RATES	112
XV. THE GIBSON PARADOX	119
XVI. CONCLUSION	123

PRELIMINARY

It can hardly be gainsaid that, in an insane and chaotic world, a deliberate, concerted monetary policy is one of the few sane methods left to the rulers of society to anticipate, check and alleviate the industrial maladies to which the world is periodically liable. Used with precision and sense, it is undoubtedly a great stabilising force; while, contrarily, if mishandled, it can as certainly be a disruptive one. The great problem of banking is a twofold one: in the first place, some measure of agreement has to be arrived at as to what should be the goal of monetary policy; secondly, the question must be decided as to what exactly has to be done to reach that goal. The relation between prices and production, the questions of appropriate outputs, of appropriate fluctuations of output and such-like issues are of far wider significance than can easily be dismissed within the compass of a few pages or with superficial reasoning. However, it appears to me that in this modern, fast progressing world, in which there is not only an actual plenty but still greater opportunities of plenty, we must devise a monetary technique which will make available to us the means of realising all those potentialities of production and wealth. It is my belief that in this realisation the economics of "dynamic equilibrium" is capable of playing a most distinguished part. It is rather unfortunate that precisely where monetary theory may be expected to come to the aid of practical banking and help it formulate some rational policy of control over the movement of prices and production, the theory is shrouded in confusion and controversy. Far be it from me to suggest that there are not operating upon the monetary and industrial structures, real and physical forces, whose scope and magnitude it is not always possible to foresee, but that does not mean that monetary authorities have not a function to perform in the scheme of industrial affairs. It is impossible to entertain the

suggestion that the monetary machine should be left to find its own manner of operation and take its own natural course: such *laissez-faire* is unthinkable for a mechanism which is capable of both so much good and evil.

It would be idle to pretend that on such highly controversial issues it is possible for anyone to say the last word. My endeavour in the following pages is to clear up certain theoretical questions connected, firstly, with the relation between interest rates and prices, which undoubtedly forms the central theme of practical banking policy, and, secondly, with the ultimate objective of monetary policy and the methodology of monetary control. Incidentally, I have been compelled to delve into some adjacent problems, such as the theory of barter, the nature of capital and so forth, but in each case I have confined myself only to the relevant parts of such theories without indulging in a rambling rigmarole of polemical economics.

CHAPTER I

THE ACTIVE AND PASSIVE ASPECTS

§ 1. THE relation between interest rates and prices is not in any sense an original issue. It has provided subject matter for controversy and investigation from the times of Adam Smith, Thornton and Ricardo down to our own. But although it forms the central issue of the theoretical aspects of banking policy, it has not yet been quite satisfactorily tackled either by the classical or by the contemporary writers. No attempt is made here to trace the doctrinal history in regard to the relation between interest rates and prices, although this is a tempting field for research. Only the present status of the theory is investigated ; consequently, in the initial theoretical survey, only those representative theories have been examined which seem to hold the fort. Mr. Keynes, in his *Treatise* (Chapter 13), has recently summarised the views of some writers on the *modus operandi* of the bank rate, but has done so more to emphasise his own differences with them than to critically examine their contents. It must be conceded that the whole theory of the interest-prices relation is still to a great extent in a nebulous condition. Anyone who goes through the relevant portions of the several text-books on monetary theory is bound to get distracted with the variety and confusion of the terms and notions formulated in them to represent the interest-prices relation. Thus, to-day, we have the "real" rate of Professor Fisher ; the "true," "proper," "normal," or "equilibrium" rate of Professor Cassel ; the "natural" or "normal" rate of Wicksell ; the "natural" rate of Mr. Keynes ; the "equilibrium" rate of Dr. Hayek ; and the latest interesting addition made by Mr. Sraffa (*Economic Journal*, March 1932) of the so-called "commodity" or "natural" rates based on spot and forward dealings. To some extent, the confusion is only verbal and superficial, but to a greater extent the

differences are differences of methods of approach, of underlying assumptions and of logic. I have attempted here to elucidate and examine the proper significance of each of the above concepts and their bearing on the theory of prices. Only after examining the validity and the relevance of these various concepts would it be possible to construct a plausible theory of the interest-prices relation and arrive at a correct estimate of the factors involved in the solution of the monetary problem.

§ 2. In the theory of this relation, it may be useful to start from two points for our investigation ; for, the relation under discussion has a double aspect. Not only does the interest-rate mechanism exert an *active* and positive influence on the price-level, in so far as it is deliberately operated with a view to management, but it itself, in its turn, is automatically and, in the absence of such authoritarian management, *passively* affected by price fluctuations, which interact on the conditions of demand for and supply of loan funds both ways. I propose to call the former the *active* and the latter the *passive* aspect of the interest-prices relationship.

Thus, with regard to the *passive* aspect, we can inquire what effect rising or falling prices have or would have on the interest rates, assuming that the banking system was not deliberately interfering with the interest-rate mechanism by forestalling or counteracting the trend of prices. A general rise of prices, for example, might somewhat increase the saving¹ capacity of the entrepreneur class at the same

¹ Before proceeding to our main task, I shall first explain here some terminological complications, as I think it is particularly risky in this branch of economic science to start without such prolegomena. There is ample reason to believe that in monetary discussions terminological differences have done even more mischief than disparate assumptions.

I prefer to follow Keynes in defining "saving" as the act of refraining from spending money income on consumption goods. Thus "savings" will be the result of such acts and will actually consist of money, which may be either hoarded by private individuals, corporations and other bodies, or deposited with the banking system. Thus it will be clear that the so-called savings deposits in the banks at any time have no definite relation with the "savings" which have been made during a given time-unit. The savings deposits ordinarily comprise not only that part of current "savings" which may have been deposited with the banks, but also the unused part of past "savings," spread over a series of time-units, and the Replacement Fund which is being continually replenished for the purpose of keeping the "real capital" intact.

"Lending" implies the act of transferring money, whether saved or

time that it reduced that of the owners of factors of production. A general rise of prices, in so far as it is associated with expansion of production, moreover, might, by increasing the total social capacity to save, make available more free and loanable purchasing power than before and to that extent enable the lending system to extend a larger volume of credit. This influence on the supply side, however, cannot

newly created or drawn from the Replacement Fund, by an individual, corporation or bank to an enterpriser or consumer at a fixed rate of interest for a stated period or perpetually. If the lending is for a stated period, it involves repayment of principal; if perpetually, it implies only perpetual payment of interest. The result of this kind of acts, *i.e.*, the money lent out, is the "loans," while the money which is capable of being so lent out is called the "loanable funds."

"Investing" is the act of purchasing "real capital" and "investments" are the money which is so spent in purchasing. Purchasing of securities on the stock exchange oftentimes implies "investing" in this sense, but does not exactly correspond to it. For one thing, a considerable amount of investing goes on without there being any dealing on the stock exchange; for another, a temporary speculative boom in securities may not lead to "investing" at all, and, on the other hand, a temporary bearishness may not contract it. The use of the term "investments," in lieu of securities, is, therefore, to be regarded as merely a rough way of putting it.

It will be immediately obvious that savings, loans and investments during a given unit of time need not and do not, as a fact, correspond at any time, because they are three distinct sets of acts performed by entirely different people, relying on varying sources as to the supply of money. Suppose, for example, we give an algebraical value x to savings, and y to loans (out of which y_1 is lent to investors and y_2 to consumers). Then, *social* savings will be not x but $x - y_2$. Thus, the savings-investments equilibrium can only be achieved if the quantity of money represented by y_1 is made equal to that represented by $x - y_2$. This illustrates also the fact that all loans are not investments. Nor, on the other hand, are all investments loans, as it is quite possible for an individual to put his own savings directly into investments without the intervention of the banks. There is one more implication of the foregoing statement, *viz.*, that the banking system, which forms a large proportion of what I intend to call the "lending" system, does not receive all savings into its vaults for deposit, and on the other hand, it is in a position to lend considerably more than what is deposited with it. It is this fact that places the banking system in a privileged position in the money market, as an arbiter of policies and controller of financial movements. Its importance will be manifest when we come to discuss bank-rate policy in relation to application of the natural-rate theory.

Then, there are the three systems corresponding to the above three acts: *viz.*, firstly, the saving system, which includes not only private savers but Government Sinking Funds, corporations, and other savers-by-proxy; secondly, the lending system including not only the banking system but private lenders and self-financiers; and thirdly, the investing system including the borrowers on the stock exchanges, Governments, corporations, Restriction Schemes, etc., which perform the function of buying investment goods of all kinds. The three functions, as well as the three systems, overlap in actuality, but, in theory, they are distinguishable.

be considerable in the *passive* aspect of the interest-prices relation. The influence of rising prices, relatively to costs, can, on the other hand, be unmistakably visualised from *the viewpoint of the demand* that rising prices and prospects of profits create for loanable funds. The same is true, *mutatis mutandis*, of the effects of falling prices on the level of interest rates. Falling prices and shrinking profits or actual losses (starting from an equilibrium position) must tend to depress the demand for loanable funds and thus bring into action those forces which may be expected to lower the interest rates. These influences are irrespective of the banking system endeavouring to manage money by means of its bank or "control" rate. When the banking technique was in an undeveloped state and the usefulness of the weapon of the bank rate was, more or less, unknown, the working of this *passive* influence of prices on interest rates might be expected to be almost complete. Even in modern times, the bank-rate mechanism has never been used with either determination or enthusiasm to check price fluctuations *à outrance*. The manipulations of the bank-rate mechanism have been everywhere a part of the narrow policy of maintaining external stability, and the tendency has been, on the whole, not to lead but to be led by the currents of contemporary events.¹

In connection with the *active* aspect of the interest-prices relation, we can consider the manner in which changes in the various interest rates, actual and "conceptual,"² influence the process of price-formation. It is now elementary knowledge that the banking system can, by means of its "control" rate (better known as the bank rate), bring about a fall or rise, though of varying magnitude, in the various rates of interest prevailing at any time in the market. The power of a well-organised banking system to achieve this is beyond doubt. Similarly, it is now a principle

¹ It is in the light of this *passive* aspect, therefore, that we ought to read the well-known statistical correlation established by Mr. A. H. Gibson, between fluctuations of prices and those of the yields of Consols, as representative of the long-term rate. See *infra*, Chap. XV, for further discussion.

² By this term, I mean to designate the various theoretical rates conceived by Fisher, Wicksell and others, such as "real," "natural," etc.

accepted and put into practice by the banking world that rate-raising either causes prices to fall or at least arrests their rise, while rate-lowering either causes prices to rise or at least arrests their decline. The theory of this *active* aspect is of particular interest to the theory of bank-rate policy and is, therefore, one of the main issues to be discussed in this book.¹

Considerable confusion has been caused by failure to separate these two, the *active* and *passive*, aspects of the interest-prices relation, as will be seen by anyone who cares to go through the history of the theory of the relation. It is an empirically established fact (*e.g.*) that high interest rates are associated with rising and low interest rates with falling prices. To deduce from this, however, that a high interest rate should itself *cause* prices to rise and that a low one should cause them to fall is obviously a dangerous statistical fallacy; and yet this fallacy has haunted monetary thought for a long time. To readers of modern monetary text-books and to those who are familiar with the control operations of central banks in modern times, this would appear to be an unnecessary reminder; but I have drawn their attention to it because even now the fallacy can be seen lurking in the writings of some writers.²

¹ Statistically, of course, it is practically impossible to separate out the influences indicated by the two aspects.

² Thus, *e.g.*, Prof. J. H. Jones, speaking of the well-known statistical correlation between interest and prices (in an article in *Branch Banking*, May 1934), says: "Now if this statistical association is to continue in the future, those who state that the rate of interest rate is likely to show a downward trend in the future are also stating, by implication, that the price-level will also show a downward trend. If such proves to be the case the price policy of the Government will have failed, and we shall once more be faced with the problem of reducing salaries and wages in sheltered employments to a level bearing the appropriate relation to the level prevailing in those industries producing commodities and services the prices of which are immediately determined by world conditions. I know at least one of the writers, who forecast a downward trend in interest, also assumes that the statistical connection of the past, between interest rates and price-levels, will be broken. And although I see no evidence in support of his view, future events may prove him to have been right." This is not a solitary example; many other writers are baffled by this correlation and see in it a possible obstacle in the way of banking control.

I should add here that the statistical correlation between interest rates and prices is negative in our *active* aspect, and positive in our *passive* aspect, though in actuality the intermingling of the two correlations might not show the factors at work in their correct perspective.

CHAPTER II

FISHER'S REAL RATE

IN this and the following few chapters, I shall deal *seriatim* with the "conceptual" rates suggested by monetary theorists, either as the "ideal" rates to which the banking system should endeavour to align its actual rates of interest, or as criteria which it should use in the control of monetary and industrial conditions.

§ 1. To begin with, let us consider the "real" rate of interest, which is generally associated with the name of Fisher, but which is really a much older notion. Professor Fisher's "real" rate is, however, the best prevailing version and may be discussed as such. Professor Fisher's "real" rate, in the first place, is not really a valuation of *interest* alone, corrected for price-changes. His "real" rate is obtained by correcting *both the interest as well as the principal for price-changes*. Thus Fisher's theorem, which is too well-known to need much elaboration, is that with the appreciation or depreciation (s) of money in terms of commodities, the "real" rate (R) may widely fluctuate being (*cet. par.*) positive with rapidly falling and negative with rapidly rising prices. If r is the money rate of interest, then he evaluates that $R = r + s + rs$, or roughly, $= r + s$, the quantity rs being negligible. If s is negative and greater than r , R is also negative and so on. This R , however, is made up of two elements; it contains in it the appreciation (or depreciation) of the money principal and the appreciated (or depreciated) value of the money interest. Thus, for example, if the principal of £100 depreciate from 100 goods (supposing £1 = 1 good, originally) to 90 goods and £5, the interest, depreciate from the expected 5 goods to 4.5 goods, the sum-total of the result will be that the "real" rate will have fallen to - 5.5 per cent. This is, however, the joint result of the depreciation of the principal (- 10 goods) and the depreciated value of the money interest (+ 4.5 goods).

§ 2. It might be objected that this dissection of the "real" rate of Fisher is of no practical significance, because to the lender who lends and to the enterpriser who borrows, what matters when prices are fluctuating is the sum-total of results and not the separate elements in the "real" rate here distinguished. But such a view, besides ignoring the fact that clarity is in itself a good, errs in supposing that the borrowed principal is always returned forthwith by the borrower to the lender. But this is not true. In the case of long-term, *i.e.*, quasi-permanent or permanent loans, like Consols and debentures, the principal is not returned at all, or if returned is done so after such a long time that the lender's foresight as to changes in prices could scarcely affect either his willingness to lend or the industrial situation.¹ Short-term loans are indeed repayable within a period of time during which fluctuations in the price-level might well cause real transfers from the borrowers to the lenders or *vice versa*, to the extent indicated by the "real" rate. But the question arises, Is an average lender or borrower (and this question applies to both short as well as long lending) *actually* so hit or benefited to the whole extent of the "real" rate? Almost always, when the loan, whether short or long, is returned, the lender *re-lends* the principal to that same or some other borrower. He does not liquidate or spend the principal. If he did, he would find himself in a favourable or unfavourable position in terms of commodities. On the other hand, the borrower, who had returned a loan, might be hit or benefited by changes in the value of money, if, on returning the loan, he wound up his business and took a long holiday. As a matter of fact,

¹ The fact that by selling his securities on the Stock Exchange a lender may recover his long-term capital at the current market price does not indicate that capital is returned by the *original* borrower to the lender. What happens is just a change of hands for the securities in question: what one lender loses another lender gains and the transfer does not affect the borrowers *vis-à-vis* the lenders at all. Professor Fisher, we are told, once informed a League of Nations audience, that "in a period of six years in the United States alone capital to the amount of forty milliards of dollars has been transferred from one section of the community to another section." But he could have seen that this sort of operations was not relevant to what he calls the real transfers between the *original* borrowers and lenders.

he does nothing of the kind ; he just goes and *re-borrows* an identical amount from the same or some other lender. Thus his gain or loss, too, so far as principal repayment is concerned, is altogether imaginary. The fact is that money capital, whether short or long, is a *continuous* process, a mobile fund ; it is continually embodied in real capital, either over long periods or over a series of short periods, according to the preference of the lender. Thus, the appreciation or depreciation of the principal, lent and repaid, scarcely affects the lender or borrower under ordinary circumstances. This does not mean, however, that the principal itself would not become intrinsically worthless, if there was a spectacular rise in prices, as there was in post-war Germany ; such a rise implies an inflation of the total volume of money capital. But assuming that the owners of the principal do not liquidate it by purchasing goods (and there is no need to make a contrary assumption), they will be hit only to the extent to which the money interest accruing to them falls in goods value. The debtors indeed may well repay their debts and avail themselves of the disparity between the nominal and market values of their investments. If, besides, subsequently, there is a complete revaluation of the monetary standard, as there was in Germany, the door will be for ever closed to the lenders possibly recouping their position in a future deflation of prices. But this is, evidently, only true of abnormal events ; in normal times, in which there are no such catastrophic depreciations followed by revaluations, but only cyclical fluctuations of prices, the creditors can always rely upon balancing their losses and gains due to price fluctuations.¹

§ 3. Apart from all this, the appreciation or depreciation of the money interest itself is an event of some actual importance to the lender, in so far as it is his regular income, which he probably spends, and to the borrower, in so far as it is an element of cost. If the concept of the "real"

¹ I am reminded here of Keynes's parable, though with a different construction, of the Professor and his Tailor, quoted from *Sylvie and Bruno* ; see *Essays in Persuasion*, pp. 370-1.

rate were thus confined to the evaluation of interest alone in terms of goods, it would become a living and useful instrument, like the parallel concept of real wages. Thus conceived, the "real" rate could never fall to zero or be negative. For instance, in the example given above the 10 per cent depreciation of the value of money (or, *i.e.*, roughly a 10 per cent rise in the price-level) would bring about a "real" rate of 4.5 per cent only and not - 5.5 per cent. Only the money rate of interest would be corrected for price-changes, so that the "real" rate, thus interpreted, would be $= r + rs$, and not $= r + s + rs$.

The representative borrower-enterpriser, who prospers on the difference, which is profits, between the costs and prices relevant to his products is generally regarded as benefiting at the expense of the factors of production. But you cannot say that there takes place a full transfer of real income to the extent of the profit from those factors of production to the enterpriser, and, in the same breath, insist that there is another transfer of the same size from the lender to the enterpriser, again. The former transfer is the true aspect; the latter is an illusion. But the "real" rate, freshly interpreted as the money interest itself corrected for changes in the value of money, fits in with the former transfer, as interest, like wages, is also an element of cost.

§ 4. Fisher's "real"-rate doctrine, widely accepted by many economists, if not as one possible explanation of cyclical fluctuations, in some way to be a thing to be reckoned with, has thus on examination proved, as it stands, to be an inaccurate explanation. Fisher himself, who founded an original theory of capital on the basis of the income concept, would have been the first to see clearly wherein his treatment of the "real" rate was faulty, if he had concentrated on the continuity of capital. The "real" rate doctrine has been applied in another allied topic, that of price stability in which the "debtor-creditor relation" is held to be one of the primary considerations. Even there, then, we have to extend the logic of the foregoing paragraphs and regard stability of interest payments in terms of goods as the essential fact. What damage (*e.g.*) a debtor country like

Argentina will suffer in consequence of a continuous fall of commodity prices is confined to her interest payments; for, to provide for these she will have to spare a larger part of her productive capacity. As for the principal repayments, she could very well avoid any present damage by re-borrowing to the extent of the repaid loans.

§ 5. An important off-shoot of Fisher's theorem, which also needs revision in the light of what has been said above, is the existence, to which Fisher directs attention, of "as many rates of interest expressed in terms of goods as there are kinds of goods diverging from one another in value."¹ The method of approach is similar to that of Mr. Sraffa's "commodity" rates with which we shall deal later on.² The distinction between Fisher's "real" rates and Mr. Sraffa's "commodity" rates is that while the former refer to the relation between two "spot" prices of a commodity at two different dates, the latter refers to the relation between the "spot" and the "forward" price for the commodity at the same given point of time. Fisher recognises the difficulty of having an indefinite number of "real" rates expressed in terms of different commodities. He, therefore, proposes to evaluate a single "real" rate by reference to the general price-level, which is, of course, his Cash-Transactions Standard (to adopt Keynes's phraseology), derived from $MV = PT$. This method, while it ignores the practical difficulty that the price-level relevant to the lender's expenditure and that relevant to the borrower's expenditure may considerably diverge, perhaps possesses the merit of simplicity.

It is Fisher's contention that in so far as the fall or rise in the "real" rate due to rising or falling prices can be *foreseen* by lenders and borrowers, the money rate at which lending takes place begins to adjust itself so as to keep the "real" rate at its *normal* level. Also that the "real" rate fluctuates more than the money rate. (This, by the way, is natural, seeing that he has included the correction of principal as well as interest for price-changes in his "real" rate. If he had interpreted the "real" rate in the narrower

¹ *Theory of Interest*, 1930, p. 42.

² See *infra*, Chap. VII.

sense here proposed, he would have found out that it fluctuates as much, though inversely, as the money rate.) The drift of the argument is that so long as the "real" rate is kept constant at some normal level, prices will be steady; if there is a deviation from the normal position of the "real" rate, prices will fluctuate. The inevitable conclusion is that if, *after such a deviation*, the "real" rate is rehabilitated by raising or lowering the money rate sufficiently to offset the effects of appreciation or depreciation in the value of money and bring the "real" rate to its normal level, equilibrium of prices will be re-established. Now, while it is indeed true that, if prices were stable, Fisher's "real" rate would be at its proper level, because neither principal nor interest would fluctuate in terms of goods, it is, as the foregoing argument must have shown, extremely doubtful, if the converse proposition holds good. At any rate, it is unnecessary for the money rate to be raised or lowered so much as to offset the fluctuation in value not only of interest but of principal also.

§ 6. The application of the "real" rate doctrine to practical banking policy, in the *active* aspect, would thus seem to be rather hazardous. I mention this because some authorities seem to lend their support to the notion that (*e.g.*) a 15.5 per cent money rate of interest would be necessary, if prices were rising at the rate of 10 per cent per annum, so that a 5 per cent "real" rate would emerge and keep the price-level steady, implying, of course, that in the circumstances a 5 per cent "real" rate was somehow a *normal* position. The argument underlying the notion is faulty in two respects: in the first place, neither Fisher nor his followers tell us how this normal position of the "real" rate is to be determined, unless it is the position which, by definition, keeps the price-level steady. This, however, obviously implies circular reasoning. Secondly, the belief, that the rate of interest in its interactions on business conditions acts or may act in a *compensatory* manner, ignores that prices do not move on an independent plane of their own but are intimately connected with movements of the interest rates. It is not necessary for the money rate to be

raised to such stupendous heights at all to control price-changes; for, the simple fact of the raising of the money rate above a *certain* level (which will be discussed later on) will itself arrest the rising prices and slacken their pace. The prices and the rate will then move convergently towards their equilibrium positions, the new rate continually bringing down the prices. It can easily be seen that even if the suggested version of the "real" rate is steadied, it may often be enough. While, however, so small a change as is indicated by the revised version is sufficient perhaps to *control* prices, it is not certain that it is *the* rate which would maintain their *stability*.

Fisher has verified his results with statistics which prove that "the real rate of interest tends to be high generally during a rising price-level and low during a falling price-level"; he also contends that "our analyses have demonstrated that in a decisive majority of instances, price-changes precede" changes in the interest rates.¹ Reference will be made to the former view while discussing the question of the long-period interest-prices relationship.

¹ *Theory of Interest*, pp. 443 and 425.

CHAPTER III

CASSEL'S TRUE RATE

§ 1. LET us proceed to Professor Cassel's "true" rate which he variously styles as being "normal," "proper," "equilibrium," or "natural." In his *Nature and Necessity of Interest* (pp. 166 ff.) it is defined as that rate of interest in terms of goods which would be brought about by the forces of supply and demand, if prices were stable.¹ *A posteriori*, Cassel concludes that if prices fluctuate, the "true" rate (a money rate, this time) will be that rate which will keep interest in terms of goods at its "true level." Skipping over all difficulties of logic, he jumps to the conclusion that this rate is also the rate which will keep the price-level steady. Now, it may be immediately granted as a direct proposition, that if the price-level were steady, Cassel's "true" rate might emerge; but it does not necessarily follow that the converse statement also holds good. We shall return to this point while discussing Mr. Sraffa's "commodity" rates. Cassel has, since the publication of *Nature and Necessity of Interest*, somewhat changed his notions about the "true" rate, especially under the influence of Wicksell, but his position is still rather ambiguous.

His present position, so far as one can gather from his latest statements,² is as follows. In conformity with his general theory of pricing, Cassel believes that the "true" rate is a price for the right of "capital-disposal" as determined by the forces of supply and demand in capital market. "It is the function of the rate of interest" (by which he means the market rate of interest) "to bring the demand

¹ This rate should be distinguished from Wicksell's "natural" rate, one of the tests for which is that it would arise in a non-monetary economy, as a non-monetary economy is not the same thing as a stable-money economy.

² See especially his article on "Interest Rate and Price Stabilisation" in the *Quarterly Journal of Economics*, August 1928; and *Theory of Social Economy* (1932), trans. References to pages in the text above refer to the latter.

for new capital-disposal into harmony with the supply—that is, with the newly-saved capital” (p. 437).

The true interest on capital might, therefore, be defined as that rate of interest at which *the value of money remains unaltered*. At this rate of interest just so much bank money will be *put into circulation as corresponds to the growing needs of trade*, the price-level remaining constant. The competition of bank money with savings on the capital market may be regarded as normal, and the rate of interest which keeps the capital market in equilibrium may be defined as the “natural rate” of interest. The rate-policy, which under a free standard strives to keep the price-level constant, would then be identical with a rate-policy which seeks to *bring the bank-rate [sic] in harmony with the natural rate on capital* (p. 502).¹

In his article in the *Quarterly Journal of Economics*, August 1928, again, “Stability of prices is possible only when the *bank rate* is kept equal to the equilibrium rate of interest. When this is done, the bank does not in any way interfere with the capital market which is therefore left to find its natural equilibrium.” Cassel thus appears to hold that the rate of interest regulates the supply of bank money which is regarded as categorically different from the “savings” in the capital market. The rate which is dictated by the latter quantity is in some sense a “proper rate” to which the banks may be well advised to adhere in carrying out their bank-rate policy. The distinction drawn by Cassel between the banks and the capital market will strike the reader as rather artificial, unless he were speaking of the banks as providing short and the capital market as providing long credit, which he certainly is not. What he is evidently trying to say is that the bank rate should be kept equal to that rate which is dictated by the supply of and demand for savings as distinct from loans—a distinction which unhappily is often lost sight of. Now the supply of savings does not depend either on the action of the capital market or on that of the banks; it is the result of the voluntary acts of individuals refraining from spending money income on consumption goods. Nor is *that supply wholly placed at the disposal of*

¹ Italics mine.

the capital market or the banks. Thus his remark (p. 439) that "the banks at all times may increase their advances only to the extent which corresponds to the total of the savings which at the time have been placed at their disposal," does not tell the whole truth. Nor is it any true criterion for practical banking, because the bankers themselves are unable to tell the genuine from the "created" deposits. No individual banker (*e.g.*) can see to what extent the rest of the banking system is lending and thus creating genuine or artificial savings deposits. Again, his own balance-sheet can never tell him what *are* the genuine savings deposits. Moreover, to suppose, as Cassel seems to do, that the banks are a part of the capital market only in so far as they lend the whole savings—neither more nor less—that are consigned to them and are acting extraneously when they exceed that, thus causing new bank money to "compete" with savings in the capital market, is hardly conducive to clarity of reasoning. It would be much more realistic to say that the capital market is the market for loanable funds and that the banks are a part of this huge lending system.

§ 2. Cassel's description of the function of the rate of interest as being "to bring the demand for new capital-disposal into harmony with the supply—that is, with the newly-saved capital," is indeed the kernel of the matter; it may be added that when the market rate performs this function, it coincides with the "true" rate. Again, Cassel rightly insists that the "true" rate is not the same as the "'real rate of interest,' 'a marginal productivity' and such-like things" (p. 501). Yet, on p. 437, he declares that "an abnormal increase of the production of capital must gradually restrict the possibilities of remunerative employment of capital, and thus render new capital investments less profitable." And he proceeds:

Under normal conditions this should bring about a fall in the rate of interest. However, when the rate of interest is already too low, the effect of the increased production of capital is to bring the situation of the capital market gradually into correspondence with the prevailing low rate of interest. But thereby

the capital market is once more brought into a position of equilibrium.

Here he seems to suggest that owing to a saturation of the capital market, there is a falling tendency in the marginal yield of past investment, thus causing the demand for lendings to fall off and that the "true" rate itself falls down to the level of the market rate and thus an equilibrium is again established. In an earlier edition of his work (1923, p. 479 footnote) Cassel scolded Wicksell for holding the view that a divergence between the market rate and the "natural" rate would cause a cumulative fall or rise in prices, on the above ground, *viz.*, that in such circumstances the latter itself would fall to the level of the former. On the other hand, in actuality we see that when the market rate is presumably lower than the natural rate, the demand for loans scarcely seems to satiate. On the contrary, with a rising tide of prices, it is ever on the up-grade. What is the explanation? The explanation, it appears to me, is as follows.

Cassel has either unconsciously mixed up his "true" rate with the "marginal yield of capital," or, if that is not so, wrongly assumed that the marginal yield falls off in a relevant manner. It is undoubtedly true that with increased investment the marginal yield would fall off, if prices were stable, *i.e.*, its money worth would be lower and lower; but when *prices are rising*, the money value of the real marginal yield may instead of falling even rise at times. Thus the marginal yield may remain in money higher than the market rate and there would hardly be any cessation of borrowing. Secondly, it is not always the marginal yield on existing capital but the prospective yield on future investment which affects the decision of the borrower; and the prospective yield must, based as it often is on pure psychology, continue to rise in times of boom. Consequently, instead of an equilibrium there will be a progressive rise up to the zenith of the boom to be followed by the crisis; there is no reason to suppose that this rise to a crisis will be forestalled by the "true" rate falling down to the market rate and thus establishing a premature equilibrium.

§ 3. The practical guide for finding out whether the market rate is or is not in harmony with the "equilibrium" rate is, according to Cassel, the stability of the general price-level. Now, apart from the objections that Dr. Hayek (*c.g.*) has raised in connection with a similar belief of Wicksell on the grounds of long-period considerations, to which I shall come later, it is extremely doubtful whether the general price-level of Cassel can be kept steady by a rate of interest which is equal to his "true" rate. In support of his proposition Cassel does nothing more than simply assert that if the market rate is lower than the "true" rate, people will borrow more and more; "this leads to an unnecessarily large issue of notes" and "the result is simply an inflation of the currency," and contrarily when the market rate is above the "true" rate.¹ Thus Cassel assumes away the very issue that one would expect him to prove by independent arguments or by reference to facts.

With regard to the rôle of interest in a social economy under dynamic conditions, Cassel's view is that a *rise* in the rate of interest restricts the demand for capital-disposal, and at the same time it has also a certain influence on saving. Thus, "*in this case*" proceeds Cassel,

the result is a *certain increase*² in the total amount of real capital produced, the productive forces of the society being drawn from the service of consumption to the production of real capital³ and perhaps also more intensively used. We then have what is generally called a rising tide in the cyclical movement of trade. A reaction comes when the production can no longer bear the burden of the high rate of interest. A general set-back in the rate of production of real capital takes place and a period of depression follows.

Readers of Mr. Keynes's *Treatise*, if they were not cautious enough, might feel that they are on familiar grounds here. But they would be wrong, because Cassel does indeed differentiate between saving and investment in real capital,

¹ *Quarterly Journal of Economics*, August 1928.

² *Italics mine.*

³ If the productive forces of the community are just drawn from the services of consumption to the production of real capital, there is no reason why production on the whole should increase. "More intensively used" is better.

more or less in the Keynesian style, but fails to make any further use of the distinction. He believes, as almost all continental economists do, that every act of saving is sooner or later followed by an act of investment. Consequently he comes to the paradoxical position ¹ that if the bank rate is raised, this causes more saving and also *more* (not *less*) investment in real capital, thus harbingering an upward phase of the trade cycle!

Mention may be made of two other points of Cassel in connection with our problem. In reply to the question, whether the bank-rate policy should be guided by the criterion of stabilisation of the short rate or whether it should aim at stabilising the long rate through the short rate, Cassel maintains that bank-rate policy should be handled with reference to the short rate; "then the other rates will adjust themselves to this rate and the result will be seen, in the stability of the general price-level." Thus he seems to hold that his "true" rate is a short-term concept. The second point, which he has made out, sheds some light on an obscurity which has puzzled many. "Attempts have been made," he says, "to refer the bank rate to the average return of all capital already invested. This is positively wrong." The reason he gives (in the article referred to) is quite plausible, *viz.*, that the marginal yield on the last act of investment itself depends on the volume of investments, which in their turn depend upon the market rate; thus it would be a vicious circle to say that the interest rate should be fixed with reference to the marginal yield of past investment.

¹ This answers the view propounded by several writers, among whom should be counted Åkermann, J. W. Angell, Hayek and others, that changes in the absolute volume of savings (and investments) are more important to trade cycle theory than the disturbances in equality between them. See *infra*, Chap. XIII, for further discussion.

CHAPTER IV

INTEREST AS COST

§ 1. REFERENCE may here be made to a controversy which has been raging for some time in America over the part that interest plays as a deterrent or stimulant to prices and production, in its capacity as a cost.¹ Mr. Carl Snyder, in an article on "Interest Rate and Business Cycle" in the *American Economic Review*, formulated two broad laws regarding the variability of short and long rates. First, that the variability of interest rates, among different groups or types of funds loaned, is, broadly speaking, in inverse proportion to the total amount of funds involved; and second, that the invariability of interest rates is in inverse proportion to the length of the term of the loans. He likened the short loan-fund to a "small reservoir attached to a large body of water," viz., the long loan-fund, "in such fashion that a small rise or fall in the main body occasions a very heavy addition to or drain from the small reservoir." Conversely, a rise or fall in the short rate by providing more or reducing existing funds affects the other to a smaller extent by slightly raising or lowering the level of the long loans reservoir. In regard to interest as cost, he asserted that "the actual cost of money to merchants is not a decisive factor in the business cycle," as it is "a relatively unimportant item in cost-accounting." In conclusion, he stressed the statistical fact that high money rates seemed rather to be the "sequelæ or aftermath of business expansion and speculative activity."

With this Professor Wesley Mitchell has joined issue. Mitchell sets himself two questions: "Firstly, is interest cost large enough in comparison with other expenditures and thus a major consideration in the determination of business policies? Secondly, are there important cyclical

¹ See *American Economic Review*, December 1925, June 1926, September 1926, and *Supplement*, March 1928.

fluctuations due to the rate of interest in the major fields of finance or economic endeavour, especially in those fields that have to bear the heaviest costs? " He answers the first question with a statistical analysis of the proportion of interest to taxes, net profits and sales. In 1919, the interest paid by manufacturing concerns in the United States amounted to 13·7 per cent of net profits, while that paid by construction companies was 25 per cent of net profits. In mining and quarrying it was 45 per cent. In conclusion, after examining other statistics, Mitchell remarks : " Interest cost may seem small and inconsequential if one compares it with total gross income or total operating expenses. Its real importance is best seen when one compares it with profit." Any fluctuation in the interest item, therefore, is in the short run at the expense of profits. For this reason, in his view, interest is a factor of considerable importance in the determination of business policies.

§ 2. Professor Mitchell, however, has not convinced his opponents that the interest cost is so large as to be a factor of major importance in the determination of business policies even in the sense that he attaches to them. In the first place, that part of the total interest cost which is the result of *permanent* or *quasi-permanent* contracts in respect of *past investments* in fixed capital cannot be affected by changes in the market rate of interest which is relevant only to new borrowings. Secondly, *changes* in the market rate of interest are of some importance to new and renewed borrowing, both for fixed capital replacement as well as working capital requirements; but the proportion of this will be so small as compared to the interest charges on initial and permanent obligations that the small change that would be caused within the total interest costs, by a change in market rate of interest, must be almost negligible, relatively to total profits.

I mention this controversy here, because the notion is abroad that bank-rate policy must regard interest cost as a lever in putting industry on its own feet after a cyclical *débâcle*. That this is not the *modus operandi* of the market rate of interest upon business activity should be clear from

the foregoing brief discussion and from the general treatment of the problem throughout the book. Those who still stick to the opinion that interest acts upon current business activity in its capacity as an element of cost have completely ignored the order of magnitude of the quantities involved. The true aspect, indeed, is the capitalisation aspect and it rightly, I think, forms a crucial element in Keynes's theory of investment.

CHAPTER V

WICKSELL'S NORMAL RATE

HERE we enter into the field of current controversy in the pure theory of money between Mr. Keynes, Dr. Hayek, Mr. Robertson, Mr. Sraffa and others. It would indeed be rather presumptuous on my part to essay an examination of the general theories of money and prices put forward by these authorities. My only excuse is that it is my purpose here to elucidate their theoretical positions and to study and find out exactly wherein their differences lie. Mr. Keynes has admitted that "the term 'natural rate of interest' is not altogether free from ambiguity," and in reply to Mr. Robertson's criticism of the "natural" rate, that the passages "which he criticises are not satisfactory as they stand and must be rewritten."¹ Nor is Dr. Hayek's treatment of his "equilibrium" rate quite clarifying except when he dwells on his differences with Wicksell.

§ 1. Wicksell's own theory of the "natural" or "normal" rate forms the starting point for these discussions. Let us, therefore, first briefly examine this important contribution of Wicksell.² Wicksell, a Swede, has this in common with the Austrians that he has partially built on the Böhm-Bawerkian theory of capital. Wicksell's theory that the relation between the money and the "natural" rate is fundamental to movements of prices and production was admittedly not new, as a number of writers before Wicksell, of whom Ricardo was the chief, had seen the significance of the relation. But their presentation of the notion was vague and they never really tried to develop it with a view to its incorporation in applied economics. Wicksell's

¹ See *Economica*, November 1931, p. 395; and *Economic Journal*, September 1931, pp. 403-6.

² The theory was originally stated in *Geldzins und Güterpreise*, 1898; its latest exposition is to be found in *Vorlesungen über Nationalökonomie*, vol. 2, esp. 216 ff. In addition, see his article, "Influence of the Rate of Interest on Prices" (his only contribution on the subject in English), in the *Economic Journal*, June 1907, pp. 213-20.

merit lies in the fact that he was the first to formulate a clear theory of the "natural" or ideal rate of interest. The theory has been assailed on many sides, especially in connection with the point that a "normal" rate, which kept the Wicksellian savings and investments equal, could not at the same time keep the general price-level steady. This indeed is a weak point in Wicksell's presentation, as we shall see. But in the hands of the Austrians, who have so readily assimilated Wicksell's doctrine, the theory, it appears to me, has been completely distorted.

Wicksell's theory, as originally stated in *Geldzins und Güterpreise*, proceeds thus. Apart from an occasional excursus into the Böhm-Bawerkian theory of the lengthening and shortening of the social production period (he calls it an "investment period"), Wicksell believed that his "normal" rate was the same rate as would emerge in a "non-monetary" static economy, and, moreover, that even a slight divergence between the market rate and the "normal" rate was sufficient to set in motion forces bringing about a *cumulative* fall or rise in the price-level. Divergence between the two rates was due to the fact that the demand for and supply of savings did not meet in their natural form (*in natura*), i.e., in the form of goods, but in the form of money, the quantity of which was changeable. Wicksell also described the "natural" rate as the rate (a money rate) at which the amount of current investment corresponded to the amount of currently available savings. The volume of investments evidently depended on the changes in the capitalistic structure of production, which were themselves due to the flow of money newly created by the banks which passed into the hands of the producers of investment goods.

§ 2. Wicksell's latest exposition of his views is to be found in his *Vorlesungen*, 1922 (reprint Jena 1928, German translation).¹ In this book, he seems to distinguish between (a) *Kapitalzins*, i.e., interest on capital, which is the real yield

¹ Prof. Lionel Robbins has recently brought out an admirable translation of the first volume of the *Vorlesungen* from the original Swedish. The second volume, however, which contains Wicksell's contribution to monetary science is yet to come.

in production (p. 234),¹ (b) *normaler Darlehnszins*, i.e., normal interest on loans, which is a "direct expression of the yield on real capital" (p. 218), and (c) the actual money rate of interest prevalent in the market for money loans. The second of these, viz., the "normal" or "natural" rate, is thus defined: "The rate of interest, at which the demand for loan-capital and the stock of saved resources exactly coincide, and which, *more or less, corresponds to the expected yield of new real capital*,² would thus seem to be the normal or natural rate of interest" (p. 220). The divergence between this and the market rate is the main-spring of price-activity, and the cause of cyclical fluctuations. Wicksell makes it clear that not the *absolute height* of the rate of interest, but the *difference* between the actual and the "normal" rate causes price fluctuations (p. 233). He maintains, however, that the divergence between the two rates of interest is usually caused by a movement of the "normal" rate (e.g., the prospective yield on new real capital rises, as a result of more profitable ways of applying capital in production being invented), and the actual loan rate failing to follow it at once (p. 233). The fluctuations in the prospective yield on real capital—which are thus the root cause of the so-called good and bad times—have an influence on prices only so long as they are not accompanied by a completely corresponding modification of the money rate of interest (pp. 237–8). If the banks were to lend below the "normal" rate, (a) saving would fall off, which would cause the demand for consumption goods to increase; and (b) to meet this demand, and also thanks to the cheapness of credit, there would be an increased demand on the part of entrepreneurs for labour, raw materials, etc. The incomes of the owners of the latter would rise, and this would again force up the price-level of consumption goods (p. 223). Even when the market rate is raised again to the level of the "normal" rate and an equilibrium re-established, prices will remain at a higher level (p. 224).

§ 3. From the foregoing summary, it would appear that

¹ Page figures refer to the German edition of the *Vorlesungen*, vol. 2.

² Italics mine.

Wicksell more or less identifies his "normal" rate, so far as its position is concerned, with the marginal-prospective yield on real capital. In Chapter XIII, below, I have dealt in a detailed manner with this view and tried to prove that the "normal" or "natural" rate, *i.e.*, the *ideal* position for the market rate, cannot be the same as either the marginal yield on existing real capital or the prospective yield on future investments. These latter are merely a consideration on the side of demand for loanable funds; their position is *indeterminate over a wide range* (especially that of the prospective yield) from which only those rates of yield appeal to business men which happen to be above the current market rate. In the scheme of yields, the ideal position of the market rate, *i.e.*, the "natural" rate, falls in its proper place as just causing enough investment to take place as will correspond to current social savings. Wicksell himself seems to have been puzzled over the relation between the "normal" rate and the prospective yield; for, immediately after declaring that the "normal" rate was the rate "at which the demand for and supply of loanable funds exactly coincides and which more or less corresponds to the prospective yield of new real capital," he argued that even under a system in which dealings took place directly between borrowers and lenders, without the intervention of the banks, prices would rise unless the market rate rose in accordance with a rise in the prospective yield (p. 220).

Wicksell's most distinctive contribution to the theory of interest-prices relation, however, lies in the point, which he stresses over and over again, that it is not the absolute height of the market rate but its height relative to the "normal" rate, that causes price fluctuations. The two complications of the "social production-period" and the non-monetary economy, however, which he and his followers have introduced in the theory, have not helped the analysis either of the interest-prices relation or of the trade cycle.

§ 4. I shall consider the Böhm-Bawerkian complications later on. Here I shall first deal with Wicksell's view that the "natural" rate is also the rate that is proper to a non-monetary economy (as distinct from either money, or

stable-money, or neutral-money economy), a notion which peeps out now and then in the writings of many continental monetary theorists.¹ In the non-monetary sense, the "natural" rate is apparently the rate determined by the supply of and demand for "real savings." This concept of "real savings" is, however, a sheer mirage in economic theory. No one has yet taken any pains to define clearly what these "real savings," as distinct from real investments or real capital, are. There is hardly any doubt that even in a non-monetary economy, we could not make all our saving in the form of so many goods (in the sense which, for example, Professor Cannan attaches to "saving up" in a discussion on Saving and Usury.² What we really would save would be (directly on our own initiative and indirectly by refusing to exercise our right of exchange over consumption goods) the *productive powers of the community*. It is this partial release (deliberate or otherwise) of the productive powers for the purpose of producing goods other than consumption goods, facilitated by the voluntary negative acts of individuals, or of corporations or Governments on their behalf, that constitutes the essence of saving, whether in a monetary or non-monetary economy. In neither economy, however, is there any such thing as an abstract real saving, an intangible something, but only saving in the above sense, *viz.*, free *monetary* or *goods* command over the factors of production, which facilitates a partial release of the community's productive forces for producing investment rather than consumption goods. It is wrong to suppose that saving in any economy would generally take the form of saving *up* the goods themselves. It is a common fallacy that what people save and invest is either consumption or investment goods, which are supposed to be of so *non-specific* a character as to be capable of being somehow utilised in the produc-

¹ Cf. (e.g.), Hayek who says, "The problem of cyclical fluctuations can only be solved satisfactorily when a theory of money economy itself—still almost entirely lacking at present—has been evolved, comprising a detailed discussion of all those points in which it differs from the equilibrium analysis worked out on the assumption of a pure barter economy." (*Monetary Theory and the Trade Cycle*, p. 131.)

These paragraphs should be read in conjunction with those in Chap. XI.

² *Economic Journal*, March 1932.

tion of real capital or of being utilised as real capital *per se*. We have, therefore, to conclude that this view of the "natural" rate as the rate emerging as the result of joint operation of forces of supply and demand with regard to "real savings" is untenable and that the "natural" rate can only be a rate proper to money economy. It is impossible to speak of the "natural" rate as a price determined by the demand for and supply of the productive forces released by the acts of saving, as the demand and supply in question are not capable of measurement except in the form of free purchasing power. We often read of the "normal" rate as being implied in a transaction in which (say) apples to-day are exchanged for apples to-morrow. Such a rate of interest, however, is only an illustration of the time-preference aspect of interest-theory; it is hardly a comprehensive view of interest such as is necessary for finding out the "natural" rate. On the other hand, the money or goods charge that might be made for the use of a piece of real capital would not be of the nature of interest, but of that of quasi-rent.

§ 5. We now proceed to the view, held by Dr. Hayek¹ and others, regarding the mutual inconsistency of the two functions assigned by Wicksell to his "normal" rate, *viz.*, the equalisation of savings and investments and stabilisation of the general price-level. Dr. Hayek agrees that "so long as the money rate coincides with the equilibrium rate, the rate of interest remains 'neutral' in its effects on the prices of goods tending neither to raise nor to lower them." When that is not the case there is set up a tendency for prices to rise or fall. While he concedes that this is a "correct" statement, he does not admit that if the two rates are kept equal to one another, the price-level must *always* remain steady. "The banks," he says, "could *either* keep the demand for real capital within the limits set by the supply of savings, *or* keep the price-level steady; but they cannot perform both functions at once." The reason which Dr.

¹ See Hayek, *Prices and Production*, pp. 20-8; and *Monetary Theory and the Trade Cycle*, pp. 111-16; also his articles on Keynes's theories in the *Economica*, 1931 and 1932. Cf. also, G. Myrdal's article on the same subject in *Beiträge zur Geldtheorie*, ed. Hayek.

Hayek adduces in support of this belief is that under dynamic conditions, in which there is an expansion of both production and the supply of savings, the two functions cannot coincide.

Although I substantially agree with the conclusion, I am not convinced by Dr. Hayek's reason as it stands. On carefully going through all the relevant passages, I find that what Dr. Hayek has really in mind is not simply a growth of savings due to a change-over from consumption to savings (which fact, by itself, cannot upset either Wicksell's or Keynes's theory as such a change-over can be offset by equalising new investments with the new savings), but a growth in the total productive capacity. With such a growth not only the flow of output that is directed towards investment but that directed towards consumption would also increase. In such circumstances, a market rate that just kept the value of the new investments equal to the new savings would indeed succeed in solving the short-period problem of business fluctuations, but it could not keep either the prices of the output or of consumption goods or those of investment goods steady over long periods in the absence of any insertion of new money into the system. This point must be conceded. It is true that neither the price-level of total output, nor what Wicksell and others called the "general" price-level can remain stable in circumstances of increasing output. Although Wicksell could not see this problem of an admittedly long-period character, it will have to be tackled by us in any exposition of the *ideal* market rate.¹

¹ The same objection was raised by Hayek against Keynes's theory, which runs on somewhat parallel lines. We must remember, however, that although the conclusion holds good even there, Keynes's theory is not identical with Wicksell's. Keynes's quantities of I and S are not the same as Wicksell's savings and investments, nor is Keynes's output price-level on the same footing as Wicksell's general price-level. Mr. Sraffa attempted a reply to Hayek's criticism in an article on "Money and Capital" in the *Economic Journal*, March 1932, contending that there was a "natural" rate in the sense of an average "commodity" rate, which if adopted would stabilise a price-level, *i.e.*, the price of a given composite commodity. It must be emphasised, however, in anticipation of discussion in Chapter VII, that Mr. Sraffa's "commodity" rate is not the same as Keynes's "natural" rate; nor has Mr. Sraffa proved that his "commodity" rate, which would stabilise a price-level, would also equalise the Keynesian S and I. The confusion is, of course, due to the fact that Mr. Sraffa, like others, has tacitly accepted all the four tests, which

Wicksell gave for his "natural" rate: viz., (i) stabilisation of the general price-level; (ii) equalisation of current savings and investments; (iii) the identity of the "natural" rate with the non-monetary barter rate; and (iv) its identity, again, with the prospective yield on future real capital. Mr. Sraffa has taken the third test in the above as his starting-point, but the quest of Wicksell himself was wrong. Cf. Chap. XI.

Mr. Keynes drops a hint in *Treatise*, vol. 2, p. 258, that in addition to the savings-investment or natural-market-rate balance, in an economically growing community, the supply of money may have to be increased at the "same steady rate as that of the general output, e.g., (say) 3 per cent per annum." Keynes's ideal is, of course, a long-period stability of the output price-level; but clearly an insertion of purchasing power meant for such stabilisation would be inconsistent with the management of the market rate at a level which would equalise I with S. The true solution, as will be maintained in Chaps. IX and XIII, must be to *cause* the price-level to fall with the cost-level over long as well as short periods and to adhere strictly to the $I = S$ equation, as a condition of industrial stability.

CHAPTER VI

DR. HAYEK'S EQUILIBRIUM RATE

§ 1. DR. HAYEK'S own theory of the relation between interest and prices is not stated by him anywhere precisely ; it lies scattered all over his writings. So far as I can see, Dr. Hayek's "equilibrium" rate is a rate that differs from all other rates referred to in this book. It is, in some sense, the rate which would keep the quantity of money multiplied by its velocity of circulation constant, in order that the total "effective" money should remain constant and *neutral* towards prices, *i.e.*, not exerting any active influence on their formation. Dr. Hayek goes a step further in saying that "any change in the velocity of circulation would have to be *compensated* by a reciprocal change in the amount of money in circulation, if money is to remain neutral towards prices" (*Prices and Production*, p. 107). He further points out (p. 108) that "the natural or equilibrium rate of interest . . . is incapable of ascertainment." Parenthetically, this "equilibrium" rate also appears to be the rate which would (pp. 23, 108) equalise the supply of and demand for "real capital," *i.e.*, what has been deposited with the banks as savings (p. 26) *plus*, perhaps, such amounts in addition as may have been saved but not invested (*Preise und Produktion*, p. 26). Even in spite of the "equilibrium" rate, however, "in times of optimism, it would not be possible to prevent the growth of circulatory capital outside the banks" (p. 108). This last remark would, therefore, seem to make the "equilibrium" rate merely an affair of savings deposited with the banks.

Elsewhere, in his *Monetary Theory and the Trade Cycle* (see pp. 212 ff.), Dr. Hayek has tried to make a desperate synthesis between the various concepts of "neutral" or constant money, the lengthening and shortening of the social production-period, the model system provided by barter, and, lastly, the postulation of a supposed readjust-

ment between certain "price margins" and the rate of interest. Moreover, he would require that "the proportion of current income, which at the given rate of interest is not consumed but reinvested in production, remains exactly equal to the necessary capital required to carry on production." This means that the requirement of equilibrium is not (or, perhaps, not only) that saving should be equal to investment, but that the investment should not exceed the bounds set up by what capital is necessary "to carry on production." All this is delightfully vague and convenient and Dr. Hayek can indeed congratulate himself that he has now produced a sufficient number of jig-saw puzzles on which to test his own as well as his followers' ingenuity.

§ 2. We shall first deal with the question of "price margins." Dr. Hayek believes that there is some kind of unique relation between the rate of interest and "the margin between the prices of means of production and of products," and that this relation is upset by a change in the rate of interest, which is caused by an "increase in the circulating media," while it is preserved if the rate is altered as a result of "increased saving activity." These assertions of Dr. Hayek are wholly of the nature of *ipse dixit*. When the saving activity is genuine and not "artificial," he asserts that the extension of production will have a further depressing effect upon the prices of consumption goods, "as the new products come on the market," but he is not prepared, rather inconsistently, to make a similar assumption when there takes place a similar extension of production owing to increased investment caused by an "increase in the circulating media." In this latter case, according to him there is a "net increase in the demand for consumption goods," brought about by an increased demand for investment goods, and that this prevents the price margins from narrowing down to the necessary extent indicated by the interest rate. But here Dr. Hayek omits to consider the further fact that simultaneously, on his data, there might be a greater increase in the production of consumption goods and that this development, along with the rise in prices of the "means of production" caused by increased competi-

tion among the employers, might well narrow the "price margins" to the desired extent or perhaps even more! This jargon of price margins, therefore, is scarcely of any value to Dr. Hayek's theory, as on his assumptions, the price margins, in any case, must readjust themselves to and reflect the *de facto* interest rates sooner or later. The notion of any unique relation between price margins and an equilibrium rate of interest completely breaks down when we seek an objective and independent criterion in it for monetary policy.

§ 3. The whole treatment of the "equilibrium" rate by Dr. Hayek is half-hearted and experimental; Dr. Hayek seems all the time to be aloof from it and sticks more to his "neutral" money. I have not been entirely successful in probing the mysteries of the conception of "neutral" money. I have no intention, however, to undertake a thorough-going critique of the notion in this place, but only to subject it to examination in regard to the immediate issue of the "equilibrium" rate.

It is easy for Dr. Hayek to define his "equilibrium" rate as the rate that keeps the "effective" money constant and neutral and then advocate that the banking policy should adhere to that rate, because that rate would stabilise production. The questions that must, however, be examined are, firstly, whether the constancy of money and neutrality of money are *mutually compatible* things; and, secondly, how far is it true to say that *the rate, that maintains the effective quantity of money always constant, is also the rate that makes current savings and current investments (in the Hayekian sense) equal.*

In regard to the former question, one cannot avoid the inescapable conclusion that Dr. Hayek has made a great muddle of that concept of velocity of circulation. *Prima facie*, neutrality of money seems an intriguing idea, but in actuality, the methodology underlying it is both vague as well as full of pitfalls. The rate of interest that would make compensation for changes in velocity of circulation would indubitably act on business in such a way that neutrality would be violated. For, velocity of circulation is, as often

as not, a *real* phenomenon corresponding to the turnover of goods and implies the spontaneous interaction of productive forces. To reduce or to increase the *crude* quantity of money with a view to compensating for changes in velocities would under such circumstances hardly constitute monetary neutrality ; for any such action must interfere with the flow of production in a "*belligerent*" manner. With regard to the second question raised above, as to whether the same rate could keep savings and investments equal, *and* could maintain the effective money constant *in the sense of compensating for changes in velocities*, we cannot be more certain either. What proof is there that Dr. Hayek's "equilibrium" rate would perform both the miracles at once ?

Dr. Hayek has not clearly stated in what connection the demand for his "real capital," *i.e.*, money savings, arises. In one passage (*Prices and Production*, pp. 45-56), he suggests that it is a "shift of demand between consumers' and *producers' goods*"; a little further (p. 51), he speaks of a "change in the proportion between the demand for consumers' goods and the demand for intermediate products." It is very difficult to hang one's coat on such unsteady pegs, in view of Dr. Hayek's definition of producers' goods as being equal to intermediate products *plus* the "original factors of production," *viz.*, land and labour. Presumably, however, as Dr. Hayek, following the Austrian theory, believes that a change in the rate of interest affects the length of production and not the original factors of production, we might perhaps be allowed to hang our coat on the peg of "intermediate products."

§ 4. Now, the social production-period, which comprises within itself these intermediate products, is a concept which though ostensibly consistent with the "agio" theory of interest bristles with numerous difficulties. Although Dr. Hayek professes that he is not enamoured of averages, his, as well as Böhm-Bawerk's, social production-period appears admittedly to be an average concept. It can hardly be pretended that the extension of the notion of average to the social production-period is a legitimate one, in view of the fact that the length of production-period differs from

industry to industry ; that it is not the same for different types of machinery or other forms of capital. Dr. Hayek has elsewhere complained that English economists "over-emphasise the distinction between fixed and circulating capital, which is at best one of degree." This, in a sense, is true with some of the older English economists, but in modern English works, I think, the emphasis is of the right degree. In Dr. Hayek's theory built on the Austrian ideology, on the other hand, exactly the reverse has happened, *viz.*, that the distinction has lost all its significance. Both fixed as well as working capital are included in the "intermediate products" which are supposed to be co-extensive with the social production-period. Cassel has very pertinently alluded in a passage in the *Nature and Necessity of Interest* (p. 170) to

the question how far back the process of production should be traced . . . a question which has caused much confusion in Böhm-Bawerk's theory of interest. As it is clearly impossible to examine all the causes which have, from the beginning of the world, led to a certain result, all investigations of economic life should be strictly limited to a definite period.

It is clear that the time factor in the social production-period has been a matter for much ransacking of brains among the Austrians. Neither Jevons, nor Böhm-Bawerk, who was indebted to him for the concept of social production-period, was very happy over this factor of time.

In Mr. Robertson's concept of the production-period,¹ *e.g.*, the time factor has not presented so much difficulty and we meet with much more definiteness. Its further development by Mr. Keynes has also the same merit. Past accumulation is here regarded as one of the factors of production, which contribute to the total output, a series of intertemporal events. Interest is regarded as a *contractual* payment for the services of the factor, capital, in respect of past and current loans, which correspond to the real capital and is held to be distinct from the yield in the nature of quasi-rent accruing from real capital itself. The distinction between the contractual payment and the actual marginal

¹ *Money* (1929); *Banking Policy and Price-Level* (1927).

yield of real capital is, indeed, at the basis of Marshall's theory of quasi-rents to which we adhere. Above all, the important distinction between the "factor" capital and that new capital, which forms part of the current output, may be regarded as the corner-stone of the Cambridge theory of capital. It is of the greatest value to a correct estimation of the relation of money to capital and of both to industrial fluctuations.

Böhm-Bawerk's followers seem to believe that the sole function of the rate of interest is to lengthen or shorten the social production-period. They lose sight of the fact that apart from this adjustment of the length of the production-period, a lowering of the interest rate (*e.g.*) might well increase the total volume of production without affecting the period itself. Oftentimes, again, the lengthening or shortening of the process of production may be due to technical advance or retrogression rather than to capital accumulation due to interest changes as such.¹ The rather fanciful interpretation put by them on the fact of a low interest rate stimulating capital-production, and a high interest rate discouraging it, that this in itself constitutes a lengthening or shortening of the social production-period is only a "round-about" way of expressing what a simpler and

¹ Cf. Professor Knight, "Capitalist Production, Time and Rate of Return" in *Economic Essays in Honour of Gustav Cassel*, p. 341. Knight emphatically says: "There is no sense in which the use of more capital in production can be interpreted as meaning a longer production process or more time, except that the creation of more capital cannot happen instantly"; and "It is not apparent, to the present writer, at least that the theory of the nature of capital is involved in, or sheds light on, the question of the flow of savings into investment which constitutes a major aspect of booms and depressions." Professor Knight has followed up this frontal attack with two more articles: "Capital, Time and the Interest Rate" in the *Economica*, August 1934, and "Professor Hayek and the Theory of Investment" in the *Economic Journal*, March 1935. I am not quite sure that the theory of the nature of capital is not involved in discussions of trade cycle theory. Perhaps, Professor Knight may be hinting that it is primarily a question of *quantity* rather than of the innate character of capital. Howsoever that may be, it appears to me that, as a large amount of production is of the nature of accumulation, a preliminary discussion of the exact process of capital production and of the interconnection of monetary and accumulative processes is essential to any complete theory of trade cycles. For the rest, I am in complete agreement with him and believe, further, that his criticism of the Böhm-Bawerk-Hayek position as regards the so-called "investment structure" or "social production-period" is completely unanswerable.

less obscure terminology would perhaps express more suitably. Whether we turn to Böhm-Bawerk's "concentric circles" or Dr. Hayek's production pyramids, we find this idea harped on again and again. Yet it is difficult to see what can be gained by calling this phenomenon a lengthening or shortening of the production-period, if it means nothing more nor less than additions to a greater or smaller extent to the existing stock of real capital. Although the interest rate represents the community's attitude towards the future in its relation to the present, and has time as one of its dimensions, it has no connection whatever with the time that is indicated by the "average" of the social production-period but only with time as one of the two dimensions of capital measurement (the other being discounted income). Moreover, the categories of "liquid" capital (in the Keynesian sense) and consumption capital do not fit in with the scheme of the social production-period, as they are not included in the term "intermediate products." Neither of these two categories is "reproductive" in the sense that "intermediate products" are; and yet the influence of the rate of interest on the volume of real capital invested in these forms must be considerable.

§ 5. The Böhm-Bawerkian view of the function of interest and of capital is too narrow to explain the ramifying manner in which changes in the interest rate affect production and prices. It is unfortunate that the notion of shortening and lengthening of the social production-period should have been placed on such a high pedestal by the Austrians in their treatment of the trade cycle. It would have been quite consistent with the "agio" theory, if they had concentrated instead on the *total* volume of real capital, command over which is affected by changes in the contractual rate of interest and if they had accorded a secondary rôle to the concept of social-production period, as being one of the many ways in which those alterations in the total demand for real capital work themselves out. The concept of social production-period, after all has been said, is hardly a definite concept at all. In the Cambridge theory, the process of production is co-extensive with working capital only, *i.e.*

with that part of real capital which is used up in a single operation during production. The working capital fund is merely a *vehicle* of imperfect values. The distinction between working capital and fixed capital is a real one, in that the former does not, while the latter does, contribute its accretion of value without getting fully exhausted in the process. The distinction is useful to the theory of short-period fluctuations and to that of the interest-prices relation.¹

The transition from less to more capitalistic processes (which, by the way, seems a better way of putting it) may come about, according to Dr. Hayek (*Prices and Production*, p. 45), in one of two ways: "either as a result of a change in the volume of voluntary saving (or its opposite), or as a result of a change in the quantity of money which alters the funds at the disposal of the entrepreneurs for the purchase of producers' goods." Thus the transition is held to be the joint result of changes in what saving is going on outside the banks (which, of course, is supposed to be lent out directly to investors), and of changes in the quantity of money that is in the control of the banks themselves. If the reader will glance at Cassel's distinction (*ante*) between the "money market" and the banks, he will at once notice a spiritual affinity in it towards this statement of Dr. Hayek. Cassel and Hayek evidently both suppose that bank credit is money, in some sense, different from voluntary saving and direct lending. Now, bankers' lendings are certainly money in so far as capital command is money, but we must not forget that they correspond to a part of what Dr. Hayek calls "voluntary saving" and which is consigned to the banks. This correspondence is often lost sight of by bankers and hence, indeed, as Dr. Hayek has elsewhere tacitly recognised (*Preise und Produktion*, p. 26), does the problem of monetary disturbances arise. The simple fact, however, that money is saved or consigned to the banks does not mean that it directly goes into circulation. The confusion is due to the failure on Dr. Hayek's part, as on that of many others, to

¹ It may interest the reader to know that, as may be expected, Mr. Robertson thinks that industrial progress and increased saving *shortens*, not lengthens, the process of production. See *Economic Essays, etc.*, p. 114.

remember that the three processes of saving, lending and investing are really independent of one another, although in practice they often overlap.

§ 6. Another point raised by Dr. Hayek's discussion, *viz.*, whether the change in the rate of interest results in an expansion or contraction of credit-money, which takes place as an *initial* process to be followed by a transition to "more or less capitalistic processes," or whether the transition takes place as the result of a change in the rate of interest, relatively to the real yield on production of capital, raises questions of what Mr. Robertson has humorously called, "a hen-and-egg order." It will be discussed after we have studied Mr. Keynes's "natural" rate. In the meantime, we shall first deal with an interesting side issue raised by Mr. Sraffa's "commodity" rate theory in the next chapter.¹

¹ It has not been possible, for reasons of space, to do full justice to Dr. Hayek's theories here. In an article (to be shortly published) on "Dr. Hayek's Neutral Money Doctrine," I have comprehensively dealt with the issues raised in this chapter.

CHAPTER VII

MR. SRAFFA'S COMMODITY RATE

§ 1. MR. SRAFFA'S theory of the "commodity" rate, which he developed in the *Economic Journal* (March 1932), while attempting to prove that the Keynesian theses applied equally well to the barter system, is briefly as follows. He thinks that it is possible to evaluate "natural" or "commodity" rates of interest ruling at any time in the markets by reference to the spot and forward prices of the commodity concerned. Thus, Mr. Sraffa says:

When a cotton spinner borrows a sum of money for three months and uses the proceeds to purchase spot, a quantity of raw cotton, which he simultaneously sells three months forward, he is actually "borrowing cotton" for that period. The rate of interest, which he pays, per hundred bales of cotton is the number of bales that can be purchased with the following sum of money: the interest on the money required to buy spot 100 bales, plus the excess (or minus the deficiency) of the spot over the forward price of 100 bales.

Thus, if a person buys 100 bales for, say, £100 spot and sells 100 bales a year forward for, say, £(100 - x), and the money rate is 5 per cent per annum; then the outgoings at the end of the year are £105 and the incomings are £(100 - x); so that the net cost will be £(5 + x); the bales that could be bought for this £(5 + x) would be Mr. Sraffa's "commodity" rate for cotton. Similarly, the money costs would be £(5 - x), if the forward price were £(100 + x), i.e., if there were a premium on cotton, the "commodity" rate being equivalent to that amount, £(5 - x). Mr. Sraffa's next point is that "in equilibrium the spot and forward prices coincide," for any commodity, and "all the 'natural' or commodity rates are equal to one another, and to the money rate." If the spot and forward prices for any commodities diverge, their respective "commodity rates" of interest also diverge from the prevalent money rate and

there is disequilibrium in the production of those commodities. While, however, some "commodity" rates might be falling and others rising, there is an average "commodity" rate for a composite of such commodities as we choose to combine, corresponding to the average of their diverging prices. This average weighted "commodity" rate or "natural" rate is the rate whose behaviour should be a matter of concern to the monetary authorities; it is the rate, Mr. Sraffa asserts, with which the money rate should be kept level.

§ 2. There are three aspects of this theory to which the reader's attention might be directed. In the first place: Is it in theory correct to evaluate the "commodity" rate by reference to the spot and the forward prices ruling at the same time in a market for a given commodity or group of commodities? Would it not perhaps be as well to base it on the relation between two spot prices relevant to two different moments, which indicate the borrowing and the repayment of the loan of a commodity? We have noticed that Fisher also considered the possibility of reckoning his "real" rate for each separate commodity by correcting the money rate for a change in its new spot price. Mr. Sraffa's method, however, is radically different. It draws our attention, in the main, to the fact that, in the event of disequilibrium, the spot and forward prices would diverge, thus establishing a "commodity" rate which is different from the prevalent money rate. The relation between two spot prices, on the other hand, which Fisher would utilise for his "real" rate is a *de facto* affair, on which monetary policy could only hold a *post mortem*; but here is a living fact on which we could rely for active guidance in that if disequilibrium arises the forward prices indicate it.

The second point that I wish to make out in this connection is about Mr. Sraffa's view that in equilibrium the spot and forward prices coincide for every commodity. This view needs no doubt to be slightly modified so as to allow for the cost of hedging. As Mr. Keynes has shown (*Treatise*, vol. 2, p. 143), in equilibrium the spot price exceeds the forward price, the "backwardation" amounting

to as much as 10 per cent in the case of seasonal crops. True, the forward dealer is normally in a position to offset opposite risks, but when either buyers of futures exceed sellers or *vice versa*, he bears a true risk for which he will ordinarily charge a high rate for hedging. Even when there are no such risks, he must always make a charge for his services.

The third point I have to raise is of greater importance to Mr. Sraffa's principal proposition. One can easily agree with Mr. Sraffa about his *a priori* reasoning as to the emergence of divergent "commodity" rates in times of disequilibrium. It may also be granted that, in a period of general disequilibrium, the "average commodity" rate will diverge from the market rate of interest. Thus it would follow that if this average "commodity" rate is equal to the market rate, there must be a position of equilibrium. But the question arises: How far a policy which conversely and artificially equalises the market rate with the average "commodity" rate would succeed in keeping a price-level stable? In other words, does Mr. Sraffa's theory provide us with a norm for the "ideal" position of the market rate?

§ 3. Let us consider this question a little closely. It is an established fact that when the crops of commodities are large, there are premiums of the forward over the spot price, and when the crops are short, the reverse of the condition obtains. This fact has been borne out by statistics,¹ and must also be theoretically granted. Let us take the case in which the spot price is below the forward price, *i.e.*, there is a bumper crop and the supply is large relatively to the demand. In such a case, according to Mr. Sraffa, the "commodity" rate will be below the market rate. How would a reduction of the market rate in these circumstances affect future supply? *Prima facie*, it appears that the reduction of the rate would encourage merchants to carry over the commodity in question, and this would raise the spot price and lower the forward price, so as to establish

¹ See Annals of the American Academy: *Organised Commodity Markets*, p. 87. Also, Messrs. Hardy and Lyon, "The Theory of Hedging" in the *Journal of Political Economy*, April 1923, pp. 283-5.

a normal relation between them. Similarly, raising the market rate up to a high average "commodity" rate, when there are short crops all round, would seem to discourage a carry-over of the commodities. Such a view, if it were held by Mr. Sraffa—and I cannot think of any other way in which his theory may be logically extended—would undoubtedly identify his theory with Mr. Hawtrey's theory of the trade cycle, based on the withholding and releasing of liquid stocks according to conditions prevalent in the money market. If it were not for the fact that Mr. Keynes had so elaborately shown the inadequacy of Hawtrey's explanation of the cycle as founded on this argument, it would have been perhaps profitable to point out that interest is not the only cost affecting the decisions of merchants in connection with carry-overs and that costs of warehousing, insurance, deterioration and the rest are more important factors. Even assuming that these are not variable, *changes* in the rate of interest must still be a matter of small account. Secondly, Mr. Sraffa's theory, like that of Hawtrey, would refer only to liquid stocks, which are only one of the three forms of investments. Thirdly, in spot and forward dealings, the *speculative* element is often the determinant of price fluctuations and counteraction of this by interest-rate manipulations would become a difficult affair, if not an impracticable one. Fourthly, Mr. Sraffa's "commodity" rate suffers in point of reliability as a monetary barometer by the fact that it does not relate to the actuality of business developments but to current ideas and expectations of business men as to the probable course of future production. It is questionable whether we could depend upon such data to discover the norms and equilibria of industry. Lastly—and this is perhaps the most damaging objection to Mr. Sraffa's theory—the "*commodity*" rate is calculable only when the money rate itself is given, as will be apparent from its equation, and, therefore, does not disclose any norm for monetary policy.

CHAPTER VIII

KEYNES'S NATURAL RATE

§ 1. After having thus found our way through a maze of "real," "normal," "equilibrium," and "commodity" rates, we may now consider Mr. Keynes's contribution of the "natural" rate in the light of the foregoing discussion. Keynes's concept of the "natural" rate is still admittedly in a rough-hewn state, but it is to be hoped that it will soon take a more definite shape, if it is to be at all helpful in solving the central problem of his theory, *viz.*, the divergence between the rate of saving and the rate of investment. Fortunately, Mr. Keynes has not fettered himself with the acceptance of the unnecessary complication of the social production-period as an explanation of an increase or decrease of the production of investment goods; fortunately, also, he has not built his structure on the Wicksellian theory of the "natural" rate as being a rate proper to a non-monetary policy. Keynes's "natural" rate is based on an *a posteriori* definition, as that rate which keeps the rate of saving and the rate of investment equal, so that the price-level of output as a whole "exactly corresponds to the money rate of the efficiency earnings of the Factors of Production."¹ The lack of any suggestion, however, of an *objective* test as to the real nature of this rate, apart from that it keeps the rates of saving and investment equal and, therefore, the output price-level steady (in its relation to costs),² has, as we shall see further on, led him into trouble with his critics who have identified his "natural" rate with marginal yield on past investment.

Keynes has, elsewhere, described his "natural" rate as a short-period affair, while he thinks Dr. Hayek's "equilibrium" rate is a long-period rate.³ Now, while it is un-

¹ *Treatise*, vol. 2, p. 155.

² This by definition; Keynes has not, however, devoted any attention to this side of his theory and plumps for stability of his π and P price-levels.

³ *Economica*, November 1931, p. 395.

doubtedly true that Keynes's own "natural" rate is fundamentally a rate, which belongs to the theory of short-period fluctuations, it is doubtful whether Dr. Hayek's "equilibrium" rate does, in fact, refer to the long period. In fact, Dr. Hayek, as I have said in the chapter on his "equilibrium" rate, has not developed his theory at all, and has shown a tendency to do without it; and I have a suspicion that, like that of Cassel, his view is that the divergence of the market rate from his "equilibrium" rate is not the only or adequate reason of fluctuations, but that the rates of saving and investment ("new and renewed") may coincide and still both together fluctuate, thus causing short-period disturbances in prices and production due to "transition to more or less capitalistic processes." On the other hand, it appears that Keynes himself has in mind another long-period "natural" rate though he has not said so explicitly. His treatment of the Gibson Paradox¹ lends support to this appearance. For, it is clear the "natural" rate, which, if adhered to, would keep the "output" price-level steady over short periods and prevent short-period oscillations of new investment relatively to new saving, must be a more fluctuating consideration than the "natural" rate, which was determined by long-priced norms of saving and investment. Without some such assumption of a long-period "natural" rate, one fails (*e.g.*) to understand the following passage²: "If the market rate of interest moves in the same direction as the 'natural' rate of interest but always lags behind it, then the movements of the price-level will tend, even over longish periods, to be in the same direction as the movements of the rate of interest." If the "natural" rate referred to in this passage were indeed a short-period one, the question arises, What prevents prices from falling further and further, if (*e.g.*) the market rate lags behind this "natural" rate, and how does a recession or revival at all take place?

§ 2. Owing to the fact that Keynes has not throughout adhered to the strict terminology of the "rate of investment" and the "rate of saving," and has sometimes spoken

¹ *Treatise*, vol. 2, pp. 201 ff.

² *Ibid.*, vol. 1, p. 206.

in a rough-and-ready manner of saving and investment as being in equilibrium, his readers have been confused over the exact statistical quantities involved. Some have assumed that the excess of S over I is measured by the increase of the inactive deposits in banks. Others suppose that S and I would be equal, *if the banking system were at any time lending out for investment the whole of the amount, neither more nor less, than had been deposited with it. This latter equilibrium is of a different order altogether. Savings, in this sense, correspond not only to that part of the new additions to saving made during a time-unit which is brought to the banks, but to that and old saving, released and floating; investment, in this sense, corresponds not only to the rate of investment but to that and renewed investment, the Replacement Fund corresponding to which is being continually released.* Now it may generally be assumed that if saving and investment in this sense are equalised, the rate of saving, S , and the rate of investment, I , will also coincide. But this need not necessarily be so. Thus, algebraically, supposing s is old and released saving, S the rate of saving, i the renewed investment, and I the rate of investment, then, even if $(s + S) = (i + I)$, still, S may not be equal to I , as s and i may not be necessarily equal. As Keynes is principally concerned with the pageant of events that is on the march in the industrial structure, he regards past saving and past investment as dead facts, of importance only in so far as they affect new saving and new investment. The presumption, however, may be conceded that if the rate of interest is kept to the level that equalises current social savings with current social investments, so that the whole fund of available savings is absorbed into investment, it will generally also equalise the rates of investment and saving, I and S .¹

Similar to, but not identical with, the see-saw of the rate of saving *versus* the rate of investment or the see-saw of market and "natural" rates of interest, Keynes conceives of another see-saw between the "prospective yield of fixed capital" and the market rate of interest. It is not, however, clear whether Keynes identifies his "natural" rate with the

¹ See Chap. XIII for further development of this point.

yield as many continental writers do. He just holds, and I think rightly, that the position of the market rate relative to that of the prospective yields is the main factor in the determination of investment policies and probably also, that this tells us simply how much investment would take place under given circumstances due to alteration of the market rate.

§ 3. Keynes's critics have rightly censured him for suddenly substituting fixed capital for investment goods in considering the prospective yield. I believe, however, there were several reasons why he omitted working and liquid capital from consideration. In the first place, normally, as he has pointed out in his statistical verifications, these latter two are quantitatively much too smaller than the volume of fixed capital investments. Secondly, the difficulty probably arose of exactly locating the notion of the price-level of increments of working capital and liquid capital, as also of capitalising their "yield." We have one high authority¹ telling us that in connection with Keynes's P' , the price-level of investment goods, there is no such thing as a price-level of the increment of working capital as distinct from its cost of production. I have not been able to follow the reasoning, but I cannot see any difficulty in speaking of a price-level of increment of working capital as distinct from its cost of production, if by working capital we mean definite imperfect goods in process of production. In any case, if by holding a given quantity of working capital and putting it through the process of production, there is an excess of accretion of value to it over the other costs going to the other factors of production, we can arrive at the *yield* in production from that working capital. Capitalisation, however, is one way of looking at the process, but the see-saw between market rate and prospective yield is in itself sufficient to explain the causal process underlying new investment.

Keynes has been criticised² for his supposed inconsistency

¹ Mr. Robertson in the *Economic Journal*, September 1931, pp. 398-9.

² *Economic Journal*, September 1931, p. 406, Mr. Robertson on Keynes's theory.

in first siding with Wicksell against Cassel's view, that prices could not rise continuously if the market rate remained below the "natural" rate, because "the capital market would become saturated and the natural rate of interest fall till it equalled the market rate," and then himself tacitly supporting Cassel in saying that a high market rate was "deterrent to the production of capital goods, until as a result of it, the falling off in their prospective supply has raised the money value of their prospective yield sufficiently to offset the effect of the higher rate of interest." This criticism of Keynes really proceeds from an insufficient explanation by Keynes of the relation between market rates, yields and the "natural" rate. I think it is easy to reconcile Keynes's position as follows. What Cassel was speaking of in the passage referred to was the marginal yield of the last unit of investment, which had been unduly encouraged by a low market rate. It is true if this yield fell, it would equal the market rate and further excess of investment cease. But the fact that the marginal yield had fallen does not mean that the "natural" rate would also fall to the level of the market rate; the marginal yield from past investment and the "natural" rate are not the same but, as I believe Cassel himself has repeatedly stated, quite distinct.¹ The reason that Cassel gives is quite logical,² viz., that the marginal yield of past investment itself depends upon how much investment has taken place in the past, i.e., in turn upon the history of the market rate itself. Thus the marginal yield cannot be a good guide for monetary policy, for fixing the "true" rate. Nor is Keynes referring in the passage (*Treatise*, vol. I, p. 203) to the "natural" rate as rising up so as to catch up with the market rate. Keynes's "natural" rate is not the same as the marginal yield on

¹ See, e.g., his *Theory of Social Economy* (1923), p. 479; his article in the *Quarterly Journal of Economics*, 1928, pp. 511-29; in his new edition (1932) of the former, curiously enough, Cassel has dropped the footnote in which he took exception to Wicksell's above view, and to which Mr. Robertson is alluding. On the other hand, Wicksell himself seems to have held the view, which cannot be accepted, that his "natural" rate was equal to the marginal yield on real capital. Cf. Prof. Fetter's interesting article, "Interest Theory and Price Movements," in the *American Economic Review*, Supplement, March 1927, pp. 98-100.

² In the article, *sup. cit.*

past investment ¹ or, what is often nearly the same thing, the prospective yield of new investment, but it is simply the rate which equalises the rate of saving, *S*, with the rate of investment, *I*. So far, therefore, there is little inconsistency in Keynes's thesis.

§ 4. I have referred to the above discussion because it clears up quite a jungle of misconceptions regarding the rôles played by the various entities of the market rate, the "natural" rate, the marginal yield on past investment and the prospective yield on future investment, in the process of price-formations, and because it answers in anticipation possible objections against my own interpretation of an "ideal" rate of interest. There is yet another line of criticism that might be adopted against it, just as it has been adopted in Keynes's case, *viz.*, that the "natural" rate doctrine neglects the Marshallian *K* and tends "to over-exalt the rate of interest at its expense."² In this connection arises also the relevance of the "hen-and-egg" circle, which has been cited as a weak point in the Keynesian analysis. I shall here deal with both these points.

The Marshallian *K* admittedly belonged to the income-price-level, as it was developed up till now. Although there were admittedly certain statistical errors in its practical application, as Keynes has shown in the *Treatise*, it was quite an elegant method of approach to the theory of price-indices. It is not, however, usually recognised that the method, like the Fisherian method, can be adopted to suit any price-level. The time has long gone by when we could regard the demand for a *stock* of money as being created by all the available goods in the market or elsewhere. In the formation of price-levels, it is never a question of stocks of money against stocks of goods, but always one of flows of goods being met by flows of money. And this applies equally to the Cambridge Cash-Balance as well as to the Fisherian Cash-Transactions Standard. Both these theories relate to the flow of money to the market: in one case

¹ Keynes is conscious of this; see *Treatise*, vol. 1, p. 205, for definition of the "natural" rate.

² Robertson, *Economic Journal*, September 1931, p. 404.

(Fisher's) the flow of money is investigated *after* the transactions are complete ; in the other (Cambridge formula) the same flow is investigated from an *anticipatory and pre-transaction* view-point. The difference between the two standards corresponds to that between anticipation and fact, and is due to the varying proportions of purchasing power which have to be held or allowed to pass from hand to hand with a view to transactions. Therefore, the fashion which has grown up of regarding the Cambridge theory as relating to a stock of money and the Fisherian theory as relating to a flow, must be discarded. It is merely a coincidence that the Cambridge formula has been (and conveniently too) applied to investigate the income value of money, while the Fisherian formula has been applied to a hotch-potch value of money relating to all the transactions that ever take place. Perhaps, Mr. Hawtrey's "unspent margin" comes nearest to the notion of a "stock," but even there I am not sure.

Both the Fisherian as well as the Cambridge equations, therefore, must be regarded as merely ancillary to the theory of fluctuations as giving us an insight into price-formations ; they cannot, however, be depended upon to provide any conclusive solution of the main problem of disequilibrium. In view of this, it is difficult to understand the complaint that the Marshallian K has been under-rated ; for neither that nor the Fisherian method, which is roughly its inverse, helps us to analyse so vividly the savings-investment factor, which is indeed the backbone of trade cycle theory. Mr. Robertson says :

Clinging to this rock [*viz.*, the mutual impact of the relevant flows of money and of goods], we shall find the rate of interest as one of the factors affecting the magnitude of the former flow, through affecting the Marshallian K —the desire of people to "hoard," that is, to keep command over resources in monetary form instead of embarking on the purchase of goods.

The critics of Keynes suppose that the Marshallian or Fisherian method would help us just as well to know how the various price-groups are affected by changes in bank rate or in the quantity of note circulation, and that so far

the new theory does not add to our knowledge. Now the "mutual impact of relevant flows" must always remain a truism: it must tell us that *if* so much more or less money got into circulation for the purchase of a particular group of commodities or was held in anticipation of such purchase, the prices would (*cet. par.*) be affected in such-and-such manner. But it does not tell us why and in what circumstances credit money gets into circulation; it does not explain the *rationale* of credit creation and that of borrowing and investment. There is a place for the theory of "mutual impact of relevant flows" even in the theory of Keynes, as I will try to show in the next chapter, but the distinction of Keynes lies in this, that he has enabled us to understand how market-rate manipulation leads to an increase or decrease in investment and *therefore* in the media of credit payments. No banking system can create credit instruments merely by announcing reductions in the bank rate; credit creation presupposes borrowing and borrowing presupposes either investment or consumption. The hen-and-egg puzzle, therefore, as to whether bank-rate reductions create more credit money in the first instance and then that credit money gets into circulation or whether borrowing first begins as a result of rate-reductions and thus credit money flows out in that very act, need not worry us overmuch.¹

¹ Further consideration of Keynes's position will be found in the following chapters.

CHAPTER IX

THE ENDS OF MONETARY POLICY

OUR study of the various "conceptual" rates has shown us that they are *the results of an endeavour to discover a norm for market-rate policy in order that the respective ideals, such as that money should be relatively stable in its value, or that it should be neutral (whatever that means) towards the formation of prices, or that it should be stable in its value in terms of a particular group or scheme of commodities, may be achieved.* The most fashionable ideal, however, appears to be the rather popular craze for the stabilisation of the so-called general price-level, which in its more refined versions sometimes becomes the *relative* stability of the general price-level.

§ 1. The world to-day seems full of stabilisers—not all of them of that desperate variety who would say, as Robertson once wittily, but somewhat in exasperation, put it,¹ that "it would be better to stabilise something than to stabilise nothing," but some external-stabilisers and some internal-stabilisers and all of them sincere believers in the virtuousness of price-stability. It is, I hope, not necessary here to enlarge upon the familiar fact that mere stability of the exchanges² can at the most keep the relative values of currencies in terms of one another stable; but it cannot exert any stabilising influence either upon the internal price-level of any country or its production and employment. On the other hand the theoretical worth of the doctrine of a "general" price-level, which is neither fish nor fowl but an average of averages, has been ably disproved by Keynes in

¹ *The International Gold Problem* (Royal Institute of International Affairs), p. 24. Robertson added that they should stabilise the wholesale price-level and gave his personal view that it would be still better if the price-level were allowed to fall at the rate of 3 per cent per annum

² Which is only misnamed "external stability," because mere exchange stability can never ensure stability of the external or international price-level. The international price-level expressed in terms of any currency depends upon the supply of and demand for international media of payments (e.g., *effective* gold money). Further discussion of this topic will be found in the next chapter.

Book I of the *Treatise*. Still we find people passionately clinging to the ideal of a stable *value* of money in terms of things *in general*.¹ They are, indeed, the Rip Van Winkles of monetary science who still believe that mere stabilisation of the general price-level would secure for us the stability of employment or the stability of consumption. An undue preoccupation with the *value* of money² has screened off from their minds the importance of the equilibrium of costs and prices, *i.e.*, of the rates of savings and investments, which is so essential to the maintenance of industrial equilibrium. They seem to be unaware that the time has arrived when we should give up worrying as to what is happening to the value of money as such and subordinate that end to the greater end of industrial stability.

§ 2. The reason why considerations of the value of money were predominant in monetary discussions in the nineteenth and the early years of the twentieth century, is to be attributed, I think, to the fact that economists were then mainly preoccupied with problems of consumption. Marshall, *e.g.*, writing in the days of Victorian and pre-war prosperity, of rising prices and adolescent trade unionism, voted for falling prices, looking at it from the angle of the wage-earner consumer; in his analysis the significance attached to the rather hard doctrine that falling prices put the employer on his mettle, etc., is only of the secondary order. Even when stability of the purchasing power was preached, a principal motive appears to have been to safeguard the consumer. Then, the debtor-creditor relation was brought into the discussion and Fisher's real-rate theorem exaggerated the fears of stabilisers who spoke of the "dead hand" of usury in regard to the long-term contracts for payments of debts. With the advance of statistical science and practice, a further stride was made and rising prices

¹ *E.g.*, Sir Basil Blackett in his *Planned Money*, p. 87: "The ideal which is sought after is a local currency which will provide a measure of value which can be spoken of as a yard-stick without the metaphor being hopelessly strained."

² I agree so far with Dr. Hayek (see his *Monetary Theory and the Trade Cycle*, *passim*) in this connection that the problems of monetary control do not call for a consideration merely of the price aspect of money. I do not, however, subscribe to his other views arising from this.

came to be associated with good and falling prices with bad trade. All the ingenuity of theoretical analysis was pressed into the service of economic science to explain this correlation. Even to this day the correlation between prices and trade remains a dominating consideration in the discussions on monetary and industrial policy.

§ 3. In the meantime, however, a noticeable change has gradually come over the minds of many economists and new ideas are coming to the forefront of discussions. To-day with a rapidly growing world productivity and with falling costs and prices, there is a shift from the problems of consumption to the problems of production and we are not regarding stability of purchasing power of money as an end in itself but as subservient to the stability of production and employment, because the mischief caused by industrial instability is many times greater than that caused by the other. This changed outlook has been emphasised by the fact that while mere stability of consumption would not give us industrial stability in a progressive society, industrial stability does imply adequate distribution and consumption under penalty of serious disequilibrium. For example, continuous stabilisation of industry over a series of time-units on the lines of the $I = S$ theory¹ might withhold the fruits of capitalistic progress from the consumer for any length of time, if under a system of highly unequal distribution capital accumulation proceeded rapidly; but the ultimate fruits of this development cannot be perpetually withheld from the less fortunate sections of mankind. Sooner or later, capitalism must unbend under penalty of the very collapse which such management (under the $I = S$ theory) may try to avert. The $I = S$ theory, unflinchingly carried out, therefore, is not inconsistent with the ultimate maximisation of consumption, merely because it aims at the maximum employment of resources for the maximum production of goods and services.

This change, therefore, in the mental outlook of economists, I think, is a healthy sign and future developments

¹ This is a brevity for the Keynesian doctrine of savings-investment parity.

of monetary theory must lie along this new preoccupation. In the new discussions, the question, How should the price-level behave? would still remain an important one. The answer is not easy. We have been taught by text-book writers the pros and cons, the advantages and disadvantages of rising, stable and falling price-levels, and although there are certain erroneous notions still held in various quarters as regards these issues, and although oftentimes either the order of magnitude or the sequence of cause and effect is lost sight of by many, we may say the cases, for and against, have been fairly and sincerely stated. It is not my purpose in this book to discuss these things anew and cover familiar ground for the sake of the knowledgeable schoolboy; for, I believe, it is now necessary to re-vamp the whole theory in view of the new importance attaching to the concept of the costs-prices equilibrium in current discussions. This is implicit in the $I = S$ theory of Keynes, *e.g.*, although insufficient stress has been laid by him as well as by others on that aspect.¹

§ 4. One possible way of achieving the costs-prices equilibrium over long, if not short, periods would be to allow costs to take their own course and let prices follow like a tottering mass. This, perhaps, is what is partly intended by Dr. Hayek in his "neutral" money doctrine. This kind of philosophy would, however, be a combination of agnosticism, nihilism and monetary pessimism, and it would be an utterly imbecile doctrine to follow in view of the tremendous waste of human and capital resources caused by the short-period fluctuations of price-levels relative to the cost-level mechanism. For there is no guarantee in falling costs that prices, even if left to themselves, would follow suit. The price mechanism is not so plastic as that. It

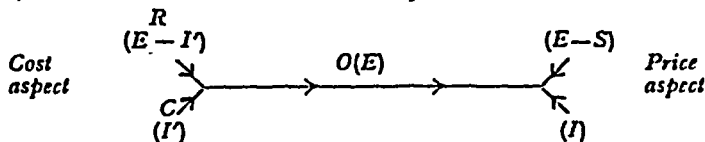
¹ Keynes himself is not quite sure of the necessity of stabilising either his π or P price-level: "For my own part, I am somewhat inclined to think without having reached a final conclusion that it is more important to have a system which avoids, so far as possible, the *necessity for induced changes*, than it is to attempt to stabilise the price-level according to any precise principles, *provided always that the rate of change in the price-level is kept within narrow limits.*" *Treatise*, vol. 1, p. 170 (*italics mine*). He appears to attach—and rightly—more importance to avoiding "induced changes."

is not necessary here to enter upon a discussion of the notorious evils of price inflations and deflations. It is just sufficient to note that monetary authorities are not so helpless in this matter and that it is possible for them to manage money in such manner as to avoid both industrial fluctuations and the consequent induced changes in the economic system. In the achievement of this end, I think, the instrument of costs-prices equilibrium is of the greatest importance.

Following Keynes, I believe that the main condition of industrial equilibrium is that windfall profits are zero, and costs = prices and $I = S$. It has been urged by many that Keynes's Fundamental Equations belong to a static theory of money. I do not deny that it is possible to apply these equations to any static theory that may be built up, but at the same time I should urge that their value is greater to a dynamic analysis of money. As I have maintained in a later chapter, however, the dynamics is not a dynamics of the actual world, which indeed is an unstable dynamics, but an ideal dynamics, postulating a series of moving equilibria over a series of time-units. The function of the "natural" rate in this scheme is, as I have said, to *maintain* the costs-prices and $I = S$ equilibrium, and *monetary control means that prices must not be allowed to lag behind costs in their downward or upward trend but must from time to time be deliberately corrected so as to correspond to cost-formations.*

§ 5. The cost aspect of Keynes's Libra¹ does not ordinarily

¹ Here it would be convenient to show in what ways the Keynesian equations mark a departure from the classical line of approach to monetary dynamics. In the following diagram, which I prefer to call Keynes's Libra, we have a brief statement of Keynes's doctrine:



The left side represents (in the above picture) the cost aspect of the Libra, the right the price aspect R , the consumption output; C , the investment output and $O (= R + C)$ the total output, represent the goods aspect. Lastly, the quantities in brackets represent the money aspect corresponding to the goods aspect in each case. Now it is easy to see that the two varieties of output, R and C , are being confronted in the market by two streamlets of money, $E - S$ and I respectively. In

fall within the scope of the banking system and perhaps for good reasons. Active interference on its part with the mechanism of wage-fixing will most certainly be resented as gratuitous and where trade unions are strong, it must be altogether impossible. Nothing short of induced changes in the earnings of factors of production brought about with the aid of Profit Inflations and Deflations can, therefore, enable the banking system to tamper with the cost-system. Such induced changes, however, presuppose harmful interactions on the structure and working of industry and cause untold misery among society. For this reason, we must admit that the main function of the banking system must be to avoid both Profit Inflations and Profit Deflations, adjust

Robertson's words, there is a "mutual impact of relevant flows." This determines their respective price-levels. Thus, $E - S$ is the money spent in purchasing R , whose costs of production are $E - I'$. Therefore, Profits on $R = Q_1 = (E - S) - (E - I') = I' - S$. Similarly, if I is the money spent in purchasing C , whose costs of production are I' , then Profits on $C = Q_2 = I - I'$. Total Profits, therefore, $= Q = Q_1 + Q_2 = (I' - S) + (I - I') = I - S$. Thus total windfall Profits, Q , must be zero, and I must be equal to S , if equilibrium is to be achieved in the production of total output. I have no difficulty, in view of discussion in the text above, in choosing between π and P for securing their equality to the costs, viz., $\frac{E}{O}$, per unit; because

if our ideal is full employment, as it must be under modern conditions, rather than stable consumption, we must choose π as our criterion. If this is kept equal to $\frac{E}{O}$, the ends of industrial stability will be secured.

It has been contended by Mr. J. E. Meade (*The Rate of Interest in a Progressive State*, p. 50) that alteration of the rate of interest would affect E , the total costs or income, and thus cause fluctuations in S , even before the new interest rate affected the investment factor. I am afraid, however, that Mr. Meade ignores the order of magnitude of the quantities involved. In Chapter IV, I have discussed the importance of interest as cost and come to the conclusion that alterations in interest rates do not affect industry through reduction in costs but in other ways. What little reduction in E is caused by lowering the rate of interest on new loans will certainly affect S , but the consequences are certainly of the order of smalls, for the simple reason that the earnings of the factor capital refer to past contracts and are fixed and unalterable, while new loans play a very small part in the scheme. Secondly, even supposing they were considerable, they would necessarily be in an opposite direction to changes in I . This still leaves room for manipulation of S and I by means of the same instrument of the interest rate.

It may be noted here that the objection, which Dr. Hayek raised against Wicksell's theory, viz., that in a progressive or retrogressive society, the rate of interest which equalised the available savings with new and renewed investments, would not at the same time stabilise the general price-level, does not apply to our theory. For, the rate of interest which equalised I and S , must keep prices and costs at identical levels, though it might not stabilise any price-level as such.

prices to costs and leave the processes of social consumption and social saving entirely in the hands of those concerned.

This is not to deny, however, that upward or downward revision of contracts is not a characteristic phase of the trade cycle, through which Nature provides her own final solution for a situation fraught with awkwardness. Such a remedy, according to orthodox economics, is provided as a kind of self-cure by the disease of maladjustment itself. It can scarcely be gainsaid, however, that the remedy in its slow process of operation causes social upheaval and political tension and is far from being a rational, active or humane solution. There is a certain school of thought, on the other hand, which believes in the efficacy of "reduction of costs and liquidation of bad debts" and which sees in this double programme the solution *par excellence* of the problem of trade cycles, and especially of depressions. This school is also sometimes associated with a morbid anti-inflationism which ill accords with the symmetry of a theory which looks upon "revision" of contracts as the basic remedy of fluctuations. If prices have fallen below costs, costs too should be brought down by reduction of wages. This is the kernel of their teaching. But the converse proposition is seldom maintained. Reduction of costs is confused with "deflation," a notion which is appropriate to *price* deflations only brought about by diminution of the media of payments; and sometimes, the preposterous suggestion is seriously made that wage reductions should be effected through price deflations, as if there was ever any direct and automatic relation between the two processes.

Let us consider this doctrine a little more closely. "Liquidation of bad debts" may be dismissed in a few words. As a remedy, it is clearly of secondary importance, as it is capable only of a certain degree of psychological impulsion. It cannot, however, exert any permanent and powerful influence upon the industrial situation. The way will, perhaps, be cleared for the reorganisation of a few bad

¹ In a progressive community these should be continually falling; in a retrogressive one they should show the opposite tendency.

concerns ; but what guarantee is there that this would lead to revival ? In a period of depression even concerns, which are not in a " frozen-up " state, find it difficult to make ends meet, because the prices they obtain happen to be far below their normal costs. Not until this relation is rehabilitated is there any chance of their ever doing well. What can liquidation of bad debts do under the circumstances ? As regards cost reduction, there is neither any logical nor empirical proof of the efficacy of this much-vaunted measure. In the first place, it is difficult, if not impossible, to bring about a proportionate and equivalent reduction in all the elements of costs, *viz.*, wages, rent, interest and normal profits. Those who preach cost reduction have generally only wages in mind, but surely mere revision of these must cause uneconomic shifts in the structure of production, according as the wage element predominates or otherwise in particular occupations. Indeed, conversion of debts is another possibility ; but this too cannot be either so all-round or so well-proportioned as to mitigate the shifts. However, to come to the other elements, who has ever thought of revision of rents and normal profits, or suggested ways to accomplish it ? It is clear, moreover, that authoritarian revision can take place only within a limited field, and, in so far as a large part of the cost-structure is outside authoritarian control, it is further clear that lop-sided revision must cause heart-burning and, more important still, must lead to a harmful and uneconomic redistribution of incomes which must punish some industries and benefit others. Secondly, what guarantee is there that reduction of costs would not lead to further contraction of purchasing power and a further price-fall, thus setting up a vicious circle ? Experience in the United States during 1930-3 shows that there is a possibility of this happening. Perhaps, however, it may be contended that reduction of costs would not lead to a correspondingly equal price-fall, but that it would establish a kind of asymptotic convergence of costs and prices towards a new equilibrium. Even supposing this were true, at what price is this to be achieved ? The social and political argument is very strong, indeed, against such

revision of contracts, whose ultimate success is so doubtful and whose immediate repercussions on life and labour are so drastic and painful. Thirdly, the empirical evidence is not such as to prove the worth of the notion. There is not a single instance of a country in which the remedy was tried and found advantageous. Recent experience in Australia is often cited, but it is now generally admitted that the Premier's Plan, in so far as it was based on cost-reduction, met with only dubious success, as a cure for the Australian slump, and that the most potent cause, which brought about revival in Australia from 1932 onwards, was the "double dose" of exchange depreciation which that country received owing to the devaluation of the Australian pound, first in terms of sterling and then in terms of gold. The case of Great Britain is also cited as a negative example in this connection. It is maintained that prior to 1931, the wage-level was unduly high and that its lack of plasticity was responsible for at least 10 per cent. of that country's unemployment. However, the other side of the shield is that wage-level was unduly high because the *price-level was unduly low*, and the price-level was lower than was justified because Britain had returned to the pre-war gold parity prematurely and at a price-level which was not in consonance with that parity. What is, then, the use of blaming the wage-level for its lack of plasticity? The experience of the United States, again, is conclusive that cost reduction may be a dangerous boomerang. Under the Hooverian régime, between 1930 and 1933, there was a wholesale reduction of wages. Yet, what was the result? The depression went on ruthlessly grinding upon wage-earners and employers alike. Then came Roosevelt with his preposterous double programme of *upward* wage revision and re-inflation. And can it be said that he succeeded less than his predecessor? The conclusion, therefore, is inescapable that cost reduction, as a remedy for depressions, has neither a theoretical nor a practical basis.

§ 6. A price-level in equilibrium with a falling cost-level (under conditions of growing productivity) does not imply at all that accumulation would diminish and that the

capital resources of the community will soon be in the process of exhaustion. With increased output, which goes with a growing efficiency of productive powers, the share of production that goes into saving will also *pro tanto* increase. The function of the monetary authority in the circumstances will be merely to see that there is a constant equilibrium between current social savings and current social investments. This it must do by managing *the investment end* of the savings-investment see-saw ; for, of the two, this is more eminently capable of authoritarian management. The question remains, however, as to what we are to do with Mr. Ramsey's theoretical *B* or "Bliss."¹ This, the theoretically ideal rate, at which a community ought to cause capital accumulation, may often be missed. It is not, however, necessarily the case that at all times the actual rate of accumulation would deviate from this ideal rate in the *minus* direction, as Mr. Ramsey believed had been happening in the case of contemporary England. It is conceivable that especially in a rich and rapidly prospering community, there may be more accumulation than is ideally right for it. Whenever the "Bliss" was thus missed, either in the *plus* or in the *minus* direction, it would perhaps be open to the monetary authority to manipulate investment, within certain limits, in such manner that the consequent Profit Inflation or Profit Deflation would provide the necessary corrective. Such manipulation, however, must be cautious, lest it got out of control.

It is my own belief, however, that instead of thus trying to manage two things at a time, *viz.*, the achievement of the "Bliss" and of industrial equilibrium, it would be better to stick to the latter and leave the process of accumulation, more or less, to the discretion of the public, because interference with this is full of pitfalls.² In a socialist community,

¹ See *Economic Journal*, December 1928 ; and Keynes, *Treatise*, vol. 2, pp. 162-3.

² We must remember in this connection that Profit Inflation does not necessarily imply rising or stable prices ; nor does Profit Deflation imply falling prices. It depends on what is happening to the cost-level. With a falling cost-level, we may well have a price-level, which is falling less rapidly, and still get conditions of a Profit Inflation. Contrarily, with a rising cost-level.

perhaps, it may be possible by a fiat to enforce adherence to the "Bliss" and still maximise production.

§ 7. There is another matter which calls for our attention, in connection with falling prices. If prices continued to fall too rapidly, though in company with costs, they would intensify the difficulties of the debtor who has borrowed with the expectation of a not unduly fluctuating price-level. In brief, the debtor-creditor relation would be strained and defaults would be frequent, if we allowed a continued fall in prices and a consequent rise in the real goods payments in respect of interest and principal later on. The "dead hand" of usury would continue to tighten upon the borrowers. This, at any rate, is economically unjustifiable and undesirable.

This problem is, to a certain extent, real and has to be faced; but it must be, first of all, remembered that it belongs mainly to the long-period economics only. It has little or no basis in the theory of the short period, as we have seen in our discussion of Fisher's "real" rate doctrine. Payments of interest alone, with which we are really concerned in the short period, do not present much difficulty due to price fluctuations. At the most, they upset the external balance of a country's economy for some time either favourably or unfavourably.¹ The payment of principal, on the other hand, presents apparent difficulty. I do not, however, think that it is of such an order as to make us think twice before plumping for the costs-prices equilibrium. Firstly, I see no reason why an appropriate share of the fruits of increased productivity should not go to the original provider of capital at the same time that the other factors of production were enjoying their own individual shares. Indeed, if prices fell by themselves and costs remained

¹ The fact that most debtor countries, like Argentina, India, Brazil and others, have been in recent years experiencing a strain on their foreign balance, owing to large increases in the real payments of interest, is due rather to the uneconomic *relative* fall of the external price-level below internal costs than to the *absolute* fall by itself. The example of Australia, on the other hand, may be cited, as that of a country which, after a bad financial career for some years, has now resumed payments owing to unexpected support gained from the rise of the external Australian price-level above Australian costs, caused by the deliberate devaluation of Australian currency.

sticky, the borrower-enterpriser would be hard put to it, when the day of repayment came ; but this is not what is implied in our theory. Secondly, the grip of the "dead hand" could, I believe, be somewhat loosened by authoritarian transferences of wealth through the agency of public finance and all of us could equally well enjoy part of the share that went to the eternal Shylock.¹ At any rate, I am not prepared to sacrifice the infinitely greater interests of industrial stability for the sake of avoiding the Shylockian evil.

§ 8. Nor need we suppose that actual changes in the cost-level are so negligible as to deserve a back seat in the array of monetary factors in the *short* period. Indeed, the *crude* cost-level in countries where trade unions are not so powerful must remain very sticky, and even where trade unions are sufficiently strong to alter wage-contracts by trial of strength, the changes are often either infrequent and jerky or not substantial enough to merit consideration. However, there is no reason to suppose that the *efficiency* cost level is equally sticky. The rate of increase in the world's productivity is, of course, extremely capricious and to the extent that it is affected by the circumstances of trade, does not fully reflect the change in true *per capita* efficiency. It is difficult to disentangle the complex effects of price disequilibria upon the trend of world production : an excessive spurt in world production brought about by actively inflationary monetary policy might seal its own fate through the reaction on price-movements later on and, in times of sagging prices, production might fall considerably below its previous norm. Still it may be pertinent to quote in this connection that

For many decades before the war, world production, according to the best available estimates, increased with remarkable regularity of trend, broken only in minor degree by successive crises. This trend of increase ran through both the period of

¹ Or, as suggested by Keynes (*Treatise*, vol. 2, p. 394, footnote), the assistance of law could be obtained in a determined attack levelled against this grip of the "dead hand" by compelling long loans to "take on the character of terminable annuities for a term not exceeding (say) fifty years, so that nothing would be payable at the end of the term."

declining prices from 1873 to 1895 and the period of rising prices from 1895 onwards. . . .¹

Production during War and post-War periods, sagging because of the depression, conceals the large increases that have taken place in the productive capacity as distinct from the actual production of the world.

The growth of the output of manufactured goods took place in many countries after the war with no, or only a small, rise, or even with a decrease in the numbers of workers employed. The rate of increase in productivity, in the years 1925 to 1929, was much greater than before the machine-producing industries expanded enormously. In the United States, there was little increase in the output per person between 1904 and 1921, but an annual increase of 3.5 per cent during the period 1922-7. In Sweden, the annual increase in the output per workman between 1920 and 1929 was 3.9 per cent; in Germany, between 1925 to 1929 it was about 5 per cent. In the United Kingdom, the output per workman in manufacture and mining increased by 10 per cent from 1907 to 1929 and by 11 per cent from 1924 to 1929.²

I shall not dwell here upon the well-known causes of the growing productive capacity of the world's population or of particular countries. It is enough to point out that underlying the excessive growth or retardation of the actual output per head, which was caused by the varying circumstances of the world's price-structure, there had been noticeable for the world, as a whole, a general upward trend of productivity per head. This increase must imply, in the absence of a constant upward revision of contracts, a constant fall in the cost-levels of various countries. *The fact that prices have not been speedily adjusted to costs, and, in fact, on several occasions, taken an upward course owing to uneconomic insertion of gold into the monetary system, has been at the bottom of the series of trade cycles that the world has gone through.*

In these circumstances, we might not perhaps be able to

¹ *World Economic Survey*, 1932-3, p. 68.

² Viljoen, *Economic Tendencies of To-day*, p. 147; see also *Recent Economic Changes*, p. 454; *The Course and Phases of the World Economic Depression*, p. 66, and *World Economic Survey*, 1932-3, Chap. III.

secure (e.g.) Mr. Robertson's¹ "appropriate fluctuations of output" by a monetary policy which aimed at actively adjusting price-levels to cost-levels from time to time; but such a policy would do a good deal at least to prevent "inappropriate" fluctuations of output. Secondly, such a policy would be as near the ideal of ultimate maximisation of production as could ever be possible; for to-day we have a world which is not only suffering from poverty in the midst of plenty but in the midst of ever-increasing potentialities of plenty. Prices having fallen out of equilibrium with costs have given rise to a plethora of unmanageable Restriction Schemes which are becoming the lions in the path of industrial progress. It is essential, therefore, that in an ordered society we should possess some mechanism which would make such impediments both unnecessary and impossible. To my mind, the theory of costs-prices equilibrium provides the necessary mechanism.

§ 9. As regards the actual technique of the costs-prices equilibrium, it is a matter for the statisticians to ferret out the most appropriate formulæ for the construction of an output cost-level $\left(\frac{E}{O}\right)$, or perhaps a number of cost-groups. I

am conscious that the task is full of theoretical and practical difficulties, but in this connection no difficulties are too great and, moreover, it is the only course suggested by monetary theory. We need not delve into the theory of costs for the formulæ in question, but first, taking a year of maximum employment as the base year, we should construct *crude* cost indices, for industry as a whole or for particular industrial groups derived from a composite of weighted indices for wages, interest, rent² and normal business profits. Having arrived at a *crude* cost index, we should

¹ Cf. his *Banking Policy and the Price-Level*, *passim*; and Pigou, "Stabilisation in Particular Industries" in *Economic Essays and Addresses*, pp. 34 ff. Also see Annals of the American Academy, *Stabilisation of Commodity Prices*, Sept. 1928.

² The difficult question to decide will be whether rent is to be regarded as a cost, (a) on national basis, or (b) in a closed system. I should regard it as a necessary ingredient of an index in either case. In any event, as rent and interest move together and are both, for industry as a whole, a smaller proportion than wages, between rent and interest, it would be merely a question of more or less *weight* being attached to each.

next investigate what is happening to productivity and construct another weighted index for changes in productivity of the factors of production. Then, dividing the *crude* cost index by the *productivity* index, we should be able to compute the *efficiency* cost index, showing changes in costs per unit of output. Thus :

	Date A	Date B
Crude cost index . . .	100	105
Productivity index . . .	100	135
∴ Efficiency cost index . . .	100	$105 \div 135 \times 100 = 77.8$

The *efficiency* cost index would indeed be open to most of the common objections raised against weighted indices, both from practical and theoretical standpoints, but I daresay it is possible to erect a workable scheme of cost-level and price-level indices which should be sufficient for the solution of the problem in hand.¹

Or, better still, if the zeal of monetary authorities is not exhausted by such mental exercises, they may engage themselves in the task of constructing cost and price indices for particular industrial groups. More often than not, this will make the problem of costs-prices equilibrium for industry

¹ I am glad Mr. Colin Clark in his *National Income, 1924-31* (pp. 126 ff.), has made an attempt to evaluate statistically various quantities in Keynes's Fundamental Equations and particularly has laid stress on the fact that the tables that he has constructed are in essence "no more than a comparison of *prices* and *costs* over a period of years, with consequent deductions as to changes in the profitability of enterprise as a whole" (p. 134). His series of indices for cost-levels and price-levels (*see below*) point to conclusions which exactly bear out the course of industrial events in Great Britain. "It is particularly interesting," says Mr. Clark, "to note how at a time when the general level of prices is falling fairly steadily, as in 1929, yet the general level of costs may be *falling more rapidly*, thus indicating a condition of over-investment." (*Italics mine.*) Since 1929, of course, as appears from Mr. Clark's figures, the output price-level has fallen below the output cost-level and bears out the conditions of Profit Deflation which have prevailed.

Years.	1924	1925	1926	1927	1928	1929	1930	1931
Costs per unit of output.	100	99.1	103.0	95.5	95.3	90.3	93.5	95.9
Price-level of output.	100	100.7	99.8	97.5	94.5	92.8	91.2	90.6

(Indices computed by Mr. Clark for U.K.)

For the ingenious method of calculation of cost-levels, I refer the reader to the source.

as a whole much easier to solve. If any particular price-group is lagging, either in the upward or in the downward direction, behind the relevant cost group, better results will be obtained by the banking system trying to pay individual attention to the particular needs of that price-group and insert so much less or more purchasing power so that it will get absorbed into that group. Such action can by no means be inconsistent with the policy of having a sum-total effect of average equilibrium for output as a whole. Nor need we fear that insertion of purchasing power into one part of the industrial structure would cause an immediate diffusion of the price-rise into other price-groups which might not need such rise. Only adjacent price-groups would be affected to some extent and it could be relied upon that adjacent price-groups did need that much rise, in view of the previous sympathetic fall in them. That because of friction the banking system is capable of such a qualitative and spatial differentiation between different industrial groups is beyond doubt ; but the technique of qualitative credit control has not yet been developed and statistical investigation will have to make great strides before any such action can be taken. If, on the other hand, it is maintained that no friction exists between the various price-groups, then the task of the monetary authorities is considerably easier, as, in that case, even the average costs-prices equilibrium will be sufficient. But such is hardly ever the case.

CHAPTER X

THE PROBLEM OF EXTERNAL EQUILIBRIUM

§ 1. IN the previous chapter, attention was directed mainly to the stability of production and employment in a closed system and the international complications of an open system were ignored. In this chapter, I propose to develop briefly the considerations which appear to me to be in view of the foregoing theory of costs-prices parity to be highly significant and appropriate in regard to the problem of "external" equilibrium. There are at least four meanings attached by economists to that penumbral expression "external stability." The most crude and at the same time the most mischievous interpretation is that which describes it as *exchange* stability, and this has already played a disproportionate rôle in the international monetary discussions. A second interpretation, which is often wrongly supposed to be more or less identical with the first, is in terms of stability of the external price level. A further refinement is that external equilibrium implies equilibrium in the balance of payments or rather in the various ingredients of it, *viz.*, goods (visible and invisible), gold and securities. This leads us to the fourth and perhaps widely accepted interpretation which makes it synonymous with *zero* gold exports. We shall see that none of these constructions satisfies the fundamental condition of industrial stability and that they are only the outcome of an undue preoccupation with problems peculiar to international, as distinct from national or independent, standards.

§ 2. To dispose of exchange stability first. In the first place, exchange stability is *not* synonymous with external stability; nor is it true to say that stable exchanges make for stable conditions of foreign trade, except to the extent that the *middleman* is protected from the risks of fluctuating exchanges. Exchange stability does not and cannot protect the producer for export, as there is no guarantee in it that

the prices which he secures for his products would have any definite relation to his costs. His costs will be largely governed by domestic considerations only, while his prices will depend upon the vagaries of the international standard, whatever that might be. Thus it is abundantly clear that the time series relating to the two will be widely and divergently dispersed and that it would be sheer accident if they moved together over any length of time. It is a narrow conception of foreign trade which looks merely to the interests of the middleman and neglects those of the home producer. Moreover, stability of the exchanges has this additional defect, that it is a "good conductor" of any outside disequilibrium of prices and communicates without the least amount of resistance all external disturbances to the internal economic structure. The problems to which this defect gives rise are often brought under the antithetical notion of "external *versus* internal stability." This antithesis, in its current interpretations, however, is full of many nebulae of suggestions which have not been always stated very clearly.

Exchange stability is to be had either by adherence to a system of pegged or managed ratios in relation to a currency of international eminence like sterling or the dollar, or by adherence to an international standard like gold. The former method is cheaper from the technical viewpoint, as it makes possible interest-earning investment in securities of the foreign monetary centre. The latter necessitates wasteful investment in a dead asset earning no interest and is only an expensive luxury. The former method causes the currency to remain parasitically attached to another currency and very often leads to the acceptance of a monetary policy which may not be suitable or even desirable from the viewpoint of the economic structure of the particular country. For example, to-day within the sterling area, the management of sterling will have perforce to be guided by purely local considerations and so as to assist the internal and foreign trade of Britain. Now, it is unthinkable that the interests of Britain would be on all fours with those of countries with widely divergent economies like Australia,

India, South Africa, Sweden, Norway and others. It would be a different matter if sterling were internationally managed, but then it would assimilate itself with a supernationally managed gold standard which is considered below.

§ 3. As regards the other method of an international standard, gold has already misbehaved not merely in the last decade or so, but throughout the latter half of the nineteenth century and the first quarter of the twentieth. It has been the root cause of the instability of the world's industry up till now. The contrary view, that it has made for stability, is born of an incorrect perspective of a mind accustomed to its traditional halo. Supernational management has been proposed for gold; but that would at the most give us exchange stability *plus* an element of international price stability. The acceptance of this proposal would depend upon the answer we give to the question whether the monetary standard should at all be international. In spite of some able advocacy of this type of a supernationally managed international standard, I feel diffident as to its success or even its desirability. Mere exchange stability without any regulation of the international prices must of course be ruled out, as such a situation is bound to lead to a progressive expansion or deterioration of the export industries and to serious disequilibria in the balances of payments of those countries which might have inelastic demands for imports; and in so far as such an unmanaged gold standard would set the pace of internal price levels, it must disrupt internal equilibria as well. Supernational management, on the other hand, might be supposed to bring about stability of international gold prices. But would such stability be the millennium? The cost-levels of different countries will move disparately, while their price-levels will dance to the tune of an international authority which cannot be expected to pay attention to individual needs. If, however, international prices are going to remain merely stable, then it may at once be stated without further argument that the problem of trade cycles *will not be solved*, for equilibrium

will be continually disrupted by disparate movements of cost-levels in relation to national price-levels whose pace is set by international management. If, on the other hand, the international standard is so managed as to remain in alignment with national costs, it will be a nice problem as to which country's costs should be considered as setting the norm. Even supposing some kind of average for costs is computed, this would still leave room for boomlets and slumpets within particular areas. There are two further arguments against the acceptance of such a standard: Firstly, that supernational management does not appear to be practical business, in spite of the Bank of International Settlements and the World Economic Conferences, and it is unthinkable that individual countries would surrender their monetary powers to a mythos of international management. Secondly, that in the present-day world, rightly or wrongly, economic welfare is preponderantly a national concern and economic policies, whether in the sphere of tariffs, of banking, of wage policy or of public finance, are shaped with the nation's interests primarily in view, while adherence to an international standard is bound to force a monetary policy upon countries which cannot be in any case consistent with the safeguarding of those interests.

§ 4. Equilibrium in the balance of payments is vague enough, but what people are generally afraid of is an *export* of gold imposing uneconomically deflationary policies on the money market internally. Of this we shall speak presently. It is not, however, generally realised that not all disequilibria in the relative magnitudes of the ingredients in the balances of payments are uneconomical, but some are very often decidedly necessary either for the economic development of particular countries or for the highly desirable enjoyment of the fruits of past foreign investment. It is indeed possible for the economy of a country to fall so out of gear that it has to live on borrowed wealth owing to the adoption of a standard of life which is incongruous with its *per capita* productivity. Such a case would be one of unproductive consumption and needs to be condemned. But that does not mean that for backward countries it is

not often necessary and advisable to allow their balances of payments to be upset in order to facilitate investment by foreigners. Such an extraordinary increase in the imports of capital does not imply international bankruptcy.

The balance of payments consists of exports and imports of (a) goods, visible and invisible, (b) gold, and (c) securities. The total money value of the exports of these three must equal the total money value of their imports: that is how the balance-sheet must balance. Under a paper standard, gold can be regarded as a commodity and included under (a), so that it merely becomes a matter of balance between the income account and the capital account. Now suppose a country enters upon a huge lending programme, so that it has a large net import of securities. This will have to be balanced by an equally large net export of goods and gold. If sufficient net export of goods, *i.e.*, Foreign Balance (*B*), is not available, gold will have to flow out. The excess of Foreign Lending (*L*) will operate by lowering the exchanges and thus causing a simultaneous flow of gold and goods. But gold being more mobile, if the country is not already in a position to increase *B* through an initial impulse coming from an expansion of foreign markets (encouraged or otherwise), gold will have to flow out immediately. Thus the causal sequence is neither necessarily always as from Foreign Lending to Foreign Balance, nor always from Foreign Balance to Foreign Lending. It all depends on whether the *initial impulse* is from the side of the terms of foreign lending and conditions of capital markets at home and abroad, or from the side of relative prices and conditions of commodity markets at home and abroad. The commodity and capital markets, or, that is to say, prices and interest rates at home and abroad, are not so intimately woven together as is commonly supposed, but are often capable of independent variations due to peculiar physical and non-monetary factors.

§ 5. The point, however, that needs emphasis here is that equilibrium in the balance of payment does not make for industrial stability as such either in domestic or in foreign-trade industries. Zero gold exports must indeed make the

Foreign Balance and Foreign Lending equal : in fact, that is what zero gold exports actually mean. But safeguarding of gold stocks is not an end in itself, but a means to a greater end, *viz.*, industrial stability. If zero gold exports are brought about by rupturing the relation between the natural rate and the market rate, they must cause a grave disequilibrium internally. Keynes has given in the *Treatise* (Chap. 21) an interesting analysis of the process by which the "international" rate (*i.e.*, the rate which makes gold exports equal to zero), the natural rate and the market rate all come together at a higher or lower parity than before, through changes in the terms of trade, and in the income and price structures of countries. But I am doubtful whether the changes which he postulates for incomes, prices and terms of trade do not belong to the long-period monetary economics, whether in fact even before the ideal triple equilibrium between the international, natural and market rates is attained, new forces are not set in motion calling for a different adjustment. In the actual world there is a constant reshuffling of forces. Although, therefore, it may be conceded that zero gold exports are a *sine qua non* of gold standard management, the mere possibility of a parity between the natural, market and international rates, which represents the ideal of such management, does not enable us to decide in favour of an international standard, be it gold or anything else.

§ 6. What should then be our guiding criteria for exchange management under a national, independent standard? This question, beyond all doubt, constitutes the main dilemma of monetary management under a national standard. Before we attempt to answer it, however, it is important that we should take a bird's-eye view of the theory of international prices and discuss the various attempts which have so far been made to give a meaning to "normal" or "equilibrium" exchanges. The discussions have assumed a new importance owing to the fact that a major part of the world is, at the moment, off gold and, therefore, in search of a correct formula for exchange management.

With his great synthetic genius, Gustav Cassel was the first to successfully essay the task of formulating a unified theory of international prices by reviving the Purchasing Power Parity doctrine and examining it in the light of contemporary empirical knowledge. Cassel initially stated his theory in its crude form when he said that the rates of exchange between any two currencies would normally reflect the relation between the internal purchasing powers of the currencies. But this crude form of his theory was open to so many objections that he was attacked on all sides and writers like Taussig pitched up the traditional doctrine of balance of payments against it. The controversy centred on the avoidable topic of causation, *i.e.*, whether the sequence of events was from prices to exchanges or *vice versa*, and on the part played by factors such as exports of capital, transport costs, tariffs and speculation. "The mutual impact of two quantities," *i.e.*, of the payments-out and payments-in, it was said, gave a more coherent and obvious explanation of the manner in which *de facto* exchanges were "determined." In view of these and other points raised by his critics, Cassel had to revise his position and restate his doctrine. He declared, and quite rightly too, that these themselves had been his assumptions, *viz.*, that the speculative element was non-operative, that there was neither prohibition nor prevention of imports and exports by fiscal measures, that there were no continuous one-sided movements of capital or great changes in transport costs. Moreover, he also insisted that he was not laying down an exchange norm for currencies in the absolute sense of the purchasing power parities ruling at a given moment of time, but that "When two currencies have been inflated, a new rate of exchange will establish which is equal to the old rate of exchange multiplied by the quotient between the degrees of inflation in the two currencies." ¹ This, the comparative aspect of his doctrine, he claimed, still remained true and it was not, he maintained, such a self-evident truism as some of his critics had tried to make out.

Keynes is the only one among monetary economists to

¹ *Post-War Monetary Stabilisation*, p. 23.

have critically examined the doctrine with an open mind. His earlier view was favourable, although he pointed out that if it was interpreted in terms of international price-levels, it became a truism, while if the non-traded price-levels were taken into consideration it would become little more than a fallacy. From comparison of wholesale price indices in various countries, however, he came to the conclusion that actual exchange rates did tend to conform to purchasing power ratios. But he discovered later on that the price indices, which he considered, being wholesale, contained a large element of international prices which had exercised a pull over the whole series in such manner as to give a semblance of truth to the theory. He, therefore, recanted his former opinion as to its scientific value. Now, on the ground that changes in real ratios of trade vitiate the theory, he has completely discarded it. Bertil Ohlin, on the other hand, has laid greater stress on factors affecting differential wholesale prices, conditions of production, conditions of local demand, conditions of transport, tariffs and other obstacles to trade, and international movements of loanable funds. This criticism, however, though quite just, ignores the fact that Cassel has made these his very assumptions and, secondly, the fact that he is concerned with "equilibrium" exchanges.

§ 7. In spite of these able criticisms, I believe, there is some part of the theory that is worth salvaging yet. The so-called truism of Purchasing Power Parity based on international price-levels does not appear to me to be either so self-evident or so devoid of usefulness as is commonly supposed. We might as well contend that the Quantity Theory of money, in spite of its modern refinements, is still a truism. Yet how many knotty problems have not been tackled with the help of that boredom! I should not, therefore, go to the length of discarding the theory of Purchasing Power Parity altogether but try to give it a constructive meaning. It must be granted that although Cassel's formula, as it stands, misses the bull's-eye, there is a sense in which we can speak of the international price-level of a country as being affected by an active internal

inflation or deflation. And here we must bear in mind the important fact, which is often lost sight of, that there is no such thing as a *unique* international price-level, unless we were speaking of a gold price-level, and that international price-levels in terms of different currencies, based on paper, might quite often fluctuate disparately owing to internal conditions of credit or note-issue circulation. Of course, international price-levels are not so liable to fluctuation as domestic or sheltered price-levels, being the resultant of international forces of supply and demand. It is, thus, strictly speaking a problem in the diffusion of international prices in the solution of which we shall have to inquire into the degree of ease and quickness with which an initial monetary change in a country tends to spread itself on to the province of its international price groups. Now it is indeed a blatant truism to say that *at any point of time*, prices of international goods would remain the same in the different countries in terms of their several currencies at the ruling rates of exchange. It is, no doubt, the business of the trader to see that that is so. But it would not be such an obvious truism to assert that as *between two points of time* and in the event of an inflation taking place in each country, the new rates of exchange would reflect the new parity given by the respective *international* price-levels. There is no reason whatever why domestic price-levels should have a say in the matter. Cassel's hotch-potch doctrine, which jumbles up international and domestic prices and puts faith in equal average changes in both, tries to prove much too much, because the average diffusion of prices is not equal in all cases. Neither in the short nor in the long run has Cassel's doctrine any validity if the price-level *as a whole* is implied; for that price-level is demonstrably not involved in a discussion of exchange norms. All the objections against Cassel's position become strong, if we take the entire price structures of countries to give the parities required. If, however, we fix our attention on international prices, we get a bed-rock for constructing a plausible theory of exchange norms. We can then follow the chain of events started by inflations and deflations of a

currency and inquire into the diffusion of these into the international price groups of the country concerned and their reactions upon the position of exchange norms. Conversely, under a régime of fixed exchanges we can watch the processes of change initiated by fluctuations of international prices abroad and communicated through the exchanges to the international price-level of the given country; also, conversely, under a system of exchange variation we can visualise the induced changes brought about in the international price-level of a country, even while there is nothing happening in the rest of the world. It is this diffusion of prices which gives our new version of the Purchasing Power Parity, so far rejected as a truism, a meaning which raises it to a new importance in the theory of international values.

§ 8. Keynes and some other writers believe that one important drawback of Cassel's theory is that it hardly takes any notice of changing real ratios of trade between countries, which so fundamentally alter the conditions of trade that it is impossible for exchanges permanently to reflect purchasing power parities. The equation of exchange, *i.e.*, the real ratio of trade for any country, is given roughly by the relation between the prices of its imports and those of its exports. Now this relation is of fundamental importance to trade, in that if it becomes adverse it is bound to upset existing equilibria to an extent which is sure to have repercussions on the rates of exchange. The argument appears to be that even without any disturbance taking place in the purchasing power parities (in the Casselian sense), the composition of the international price-level consisting of import and export prices might alter in such wise that the *de facto* exchanges would fail to correspond with the parities calculated.

Now this subject of the real ratio as affecting exchange equilibria is somewhat confusing and even Keynes has not fully elucidated his argument in the *Treatise*. Yet I have some misgivings as to the validity of the objection against the Purchasing Power Parity Theory. The objection is somewhat in the classical style and in line with the balance of payments theory of exchanges in that it incidentally

suggests that if the relative positions of import and export prices alter, the effect on the exchanges will be *via* the balance of payments. In this connection it is rather characteristic that while the revised doctrine of Purchasing Power Parity, as stated here, takes into consideration the international price-level as a whole, this objection divides up that price-level into its component parts, *viz.*, import and export prices, and considers their relation *inter se*.

Before I proceed to consider the relevancy of the objection so far as our version of Purchasing Power Parity is concerned, I should like to make two observations regarding the rôle of the real ratio in the scheme of the international price structure. Firstly, it appears to me that the effects of tariffs, of one-sided transfers of wealth and of changes in transport costs upon real ratios are somewhat exaggerated. The exaggeration is due in a large measure to the undue preoccupation of economists with bilateral trade and to their frequent neglect of the fact that international prices are determined by world-wide forces of supply and demand. In spite of some penetrating researches made into the subject by writers like Taussig, Jacob Viner, Ohlin and others, I am inclined to think that the specific effects of tariffs, loans and transport costs on the real ratio of trade have not been clearly separated from the effects on it of the general conditions of international demand and supply relating to imports and exports and from the effects upon it of *the very changes in exchange rates* induced by tariffs, loans and other factors. This perhaps is an impossible task, but the fact remains that the possibility of multiangular trade reduces the chance of the real ratio of any country being adverse or favourable due to temporary causes confined to its own economy and to its own income and price structures. This is not to deny, however, that the above factors of tariffs, etc., would not affect the *de facto* exchanges or even exchange equilibria, but such a result would be due to their action upon the balance of payments; the effects would not, in any case, be communicated *via* the *real ratio* necessarily. Secondly, I cannot help thinking that in discussions of real ratios writers have very often mixed up

three different interpretations of the notion of the "real ratio." The real ratio of trade of any country is legitimately calculated by dividing the value of its currency in terms of imports by its value in terms of exports; or by dividing the value of its currency in terms of foreign productive factors by its value in terms of local productive factors. It is also sometimes, wrongly, calculated by dividing total imports by total exports of visible and invisible goods. All these shades of meaning are often employed indiscriminately.

With these observations in mind, we shall now consider whether changes in the real ratio affect our version of Purchasing Power Parity. Tariffs or one-sided transfers of wealth might be granted to work upon the exchanges in the first instance and through them to alter the real ratio to an extent which would necessitate changes in internal income and price structures. These changes in internal price structures would then no longer permit the Casselian purchasing power parities to be reflected in *de facto* exchanges. But would they disrupt the relation between international price parities and exchanges as well? I very much doubt if they would. In my view, in spite of changes in real ratio, the truism must always remain a truism. The international price-levels in the comparative dynamic sense must give us a parity which should always conform to the exchange norms and facts. There is nothing in the real-ratio changes as such that is capable of destroying the relation between international price parities and exchange equilibria.

§ 9. What bearing has the foregoing discussion upon our quest for exchange policy criteria? It is as follows. In an international system, in which countries may be desirous of adopting exchange ratios calling for the *least amount of management* and causing the *least disturbance in the status quo of prices*, this new version of Purchasing Power Parity is capable of playing a great part. If the adoption of a particular exchange rate has caused either inflation or deflation in the country's international prices, even when prices in the rest of the world have been steady, it may be

immediately proved that the currency concerned has missed the norm of exchanges. If, however, prices in the rest of the world are also falling (or rising), adherence to exchange norms, *i.e.*, to the international standard, will give nothing but a fall (or rise) in a country's prices also. No amount of management would then help, as such management would be contrary to the ideal of international stability.

There is still another angle from which possibly we could view this question. The *Economist* of London has been recently calculating exchange equilibria for the dollar and the pound in a series of articles on the subject ¹ and has suggested a novel method, which may well be described by a quotation from the journal:

The equilibrium rate between the pound and the dollar is the rate which would put the two countries on a fair competitive footing. It is consequently the rate which would so adjust the relative costs of production that the volume of exports of each country would be sufficient and no more than sufficient to bring the balance of payments into equilibrium. In the short period, costs of production can roughly be assumed to vary with wage rates. Hence a calculation of "cost of production parity," based on the movements of wage rates, should provide a useful check upon calculation of "purchasing power parity."

This statement looks like an omnibus theorem in so far as it seeks to reconcile two really divergent aims, *viz.*, of equilibrating the balance of payments and of putting countries on "a fair competitive footing." Still it provides a new clue to exchange management in a world which is getting more and more entangled in a maze of monetary policies. The whole philosophy of exchange depreciation, at the moment at any rate, seems to be pervaded by the one ambition of capturing foreign markets and encouraging exports. Thus in many countries the relation between international prices and internal costs has been neglected to the disadvantage of internal trade, as there is little or no inflation in internal prices. This kind of tinkering with exchanges can scarcely have any permanent effects on either international trade or business recovery, as one nation's gain must be another's loss, and if all countries

¹ See numbers, 17 June 1933, 18 November 1933 and 27 October 1934.

depreciate currencies in terms of one another, they will remain just where they were, so that on balance there would hardly be any progress at all. Therefore, it appears to me that this method of calculating "cost parities" would be very suitable as a corroboration or check upon the "price parities." The only reservations I wish to make are, firstly, that to base the cost parities on the movement of wage rates, even in the short period, would give fantastic results, for the simple reason that *unit* costs, even in the short period, are very much liable to alteration when efficiency is rapidly increasing; secondly, it need not be supposed that these costs are any more precise as indications of exchange equilibria than price parities can be. Almost all the assumptions which we have to make for the price parity doctrine also apply here.

§ 10. Both the price or "purchasing power" parity thesis as well as the cost parity thesis would be helpful, if our policy was based on the ideal of a workable international standard. Neither of them is, however, useful in attacking the dilemma of external management under a national standard. Under such a standard, the only legitimate meaning we can give to external stability is the stability of foreign-trade or "unsheltered" as distinct from mere "exporting" industries. In economically advanced countries like England, France, Germany and the United States, "sheltered" or domestic industries may be expected to play a greater part than in backward countries of the East or in countries producing only agricultural or raw-material products; in the former, services of a more civilised variety may be expected to weight the domestic price-level more heavily than in the latter. This dichotomy is not, however, inconsistent with the fact that (*e.g.*) England has a larger proportion of international trade to total trade than a country like, say, India. The actual volume of international trade of any country and the character of its economy and price structure are two distinct notions. If we bear this in mind, we shall immediately grasp why a *policy of exchange variation and induced changes in international price-levels* has a greater significance for countries like

Australia, South Africa, India, Brazil, Canada or Argentina than for, say, England. For those countries have a predominantly "international" price structure and respond more quickly and readily to changes in exchange rates and in their international prices. Even in the case of countries with less "international" economic structures, external management implies manipulation of exchanges with a view to achieving stability in the foreign-trade, as distinct from exporting, industries. Looking at the problem from this angle, we should be able to see that *to achieve such stability we must manage our exchanges in such a way that our international prices would correspond to our internal costs*. Now, this objective of management is not identical with the foregoing one of cost parity but is quite different and can be used as a subsidiary weapon to keeping internal stability of the costs-price relation by reference to a tabular standard. Such management of exchanges will not be incompatible with the independent management of money with a view to internal stability, as the price groups concerned are more or less distinct, and even if at times mutually contrary aims have to be pursued, the forces of monetary friction would not permit diffusion of impulses to take place for more than a short time or to any great extent. Anyhow, it is necessary to insulate the economic system of a country against outside disturbances which under modern conditions are unfortunately too prone to occur.

Management of exchanges according to the above plan may mean deliberate lowering of exchanges on occasions and their raising on other occasions. It is wrong to argue, however, that these changes themselves would bring about alterations in internal costs so as to nullify their own good effects. A common fallacy is that depreciation of exchanges sooner or later brings about a rise in home costs through a rise in internal prices and cost of living and thus causes the "temporary" advantages to disappear. I would like to contest this view only in so far as it affects my argument and in so far as it may lead one to believe that exchange variations would affect costs in the same degree and thus cancel their own benefit. It may be conceded that

exchange variations would cause opposite and immediate changes in international prices, which may have sympathetic repercussions on domestic prices also. But it is a preposterous idea that this chain of causation would necessarily and automatically lead to alterations in *costs*. *Cost of living indices are not at all involved here*, and between cost of living and costs of production and between costs of production and prices there is a whole range of political and economic relationships based on custom, contract and parliamentary regulation, which are fairly rigid and uncompromising. Even in spite of a price-rise in imported raw materials, it is possible for costs in a country with depreciated exchanges to remain *permanently* divorced from prices. Anyhow, we have to grant that no vicious circle is set up between prices and costs due to exchange variations.

There are two more possible objections to be answered. It may be said that an ancillary exchange policy as outlined above might lead to the artificial buttressing of export industries at the expense of home industries and that such a hot-house growth of foreign-trade industries might perhaps stabilise employment but it cannot maximise the national dividend. Now, I, for my part, believe that a capitalistic system, operating within a scheme of checks and balances and of conflicting equilibria, is incapable of maximising the national dividend and all that we can do is to effect a compromise. Of all these compromises the best one appears to me to be the maximum possible employment of national resources which is at the same time as near as possible to an *optimum* distribution of those resources between alternative uses. If costs are calculated with reference to the entire group of industries, such a costs norm, if kept in parity with international as well as domestic prices, need not give undue advantage to export industries. International prices, roughly, composed of export and import prices, will be determined both by conditions at home as well as abroad and there is no fear of price manipulation, apart from the effects on prices of exchange variations. The only artificial element of difference would be that the distribution of national resources between the domestic

and foreign-trade industries might be somewhat different from what it would be under an uncontrolled system ; but we have to choose between such a distribution of resources on the one hand and the possibility of cyclical fluctuations and the certainty of international disequilibria on the other. Another objection that might be raised is that exchange variations would not succeed in producing the desired effect if two countries simultaneously pursue mutually conflicting exchange policies ; *e.g.*, if the sterling authorities seek to depreciate the pound in terms of the dollar at the same time the dollar authorities are seeking to depreciate the dollar in terms of the pound, neither will succeed. It is clear, however, that it is an average effect which has to be obtained in regard to all the other currencies and the exchange variation is not to be in terms of only one or two currencies.¹

¹ In recent times, cases, which afford examples of this policy of exchange variations for aligning international prices with domestic costs, are those of Australia and South Africa. All the economic ills from which these countries were suffering were cured with surprising rapidity as soon as they went off gold and reduced their exchanges, thus facilitating the abovesaid costs-prices parity. Cf. Dr. Copland's Alfred Marshall Lectures on *Australia in the World Crisis, 1929-32*, and Prof. C. S. Richards's brilliant article on "Economic Revival in South Africa" in the *Economic Journal*, December 1934.

I should add that I am aware of the dangers of inflation caused by undue depreciation as well as of those of "competitive" depreciations, but here I am concerned with *rational* policy aiming at providing correctives wherever costs and prices might have diverged and not with unscientific and harmful exchange tactics. I am not preaching depreciation only ; on occasions exchange *appreciation* would also be necessary,

CHAPTER XI

IS BARTER THEORY RELEVANT?

§ 1. BEFORE attacking the main problem of this book, *viz.*, the "natural" or "ideal" rate, I must first touch upon certain side-issues which are of great relevance to it. Discussion of these has been anticipated in previous chapters, but unless we deal with them in a more comprehensive manner, they are likely to remain hindrances in our way. First of these is the question, as to what extent the theory of a non-monetary economy is likely to be useful in the understanding of monetary phenomena. In current discussions the attitude of most writers in this matter has been altogether dubious, if not misleading. Unfortunately, excepting a few passages in economic text-books, there exists no literature expounding with any degree of fulness the implications of the theory of barter, and yet the hypotheses of Robinson-Crusoe and savage economies play an inordinately large rôle in most theoretical discussions of modern economic problems. As we have seen, hypothetical considerations of a non-monetary character have been introduced in monetary theory in connection with our problem, particularly by Cassel, Wicksell and Hayek. The hypotheses, however, are not *in pari materia* but relate to distinct concepts of the non-monetary economy.¹ As those concepts are supposed to be highly relevant to the theory of the interest-prices relation, we shall analyse them a little more closely than has been possible in the foregoing pages.

§ 2. Let us begin with the Robinson-Crusoe economy. This is the typical, one-man, exchange-less economy, in which the *homo œconomicus* is supposed to be stranded on a desert island. Böhm-Bawerk makes good use of this kind

¹ Professor Robbins, by the way, has given an entirely novel meaning to the term "non-monetary" by contrasting the analysis that is necessary to determine the value of goods (in terms of money) from that which is necessary to determine the value of money (in terms of goods). Cf. *Essay on the Nature and Significance of Economic Science*, p. 81.

of economy in his chapter on the "Formation of Capital" in the *Positive Theory of Capital*.¹ Robinson Crusoe is regarded as entirely subsisting on berries. "Crusoe may save and stint as much as he pleases, he will accumulate a store of berries—goods for consumption—but that will never give him a single bow or arrow." What Crusoe will have to do, in order to get his bow and arrows, is to set free his productive powers—so many hours' labour—for the proposed formation of capital. This release may be immediate or prospective; thus, Crusoe can save a surplus of berries to-day and release to-morrow's labour or can forgo consumption of some berries by not collecting them and thus releasing a corresponding amount of labour to-day. In either case, it becomes a "saving of productive powers," for purposes of producing investment goods. Contrast this with Roscher's classical parable² of the primitive fisherman who caught three fish every day with his hands but could catch thirty with a boat and a net. Roscher, like his contemporaries and some of the moderns, falls into the error of supposing that the only inevitable way in which the fisherman can construct his boat and net, is to accumulate, at the rate of one fish a day, a stock of one hundred fish and then discontinue fishing altogether, while he makes the boat.

§ 3. From the Crusoe economy we pass on to an advanced social economy, in which there is exchange but no *medium* of exchange, in which goods are exchanged against goods. This is the non-monetary economy proper and is better known as Barter. Barter means necessarily a crude economy. Perfected barter economy would be an approximation to money economy in regard to relative valuations (assuming distribution to remain the same), though not in regard to stability. It is not, however, as is commonly believed, an entirely stable economy. Relative valuations of goods, services and other forms of wealth are liable to fluctuate therein as much as they do in a money economy. Assumptions of barter are unduly simplified when we regard

¹ Trans. Smart (1923), reprint, pp. 100 ff.

² *Principles of Political Economy*, trans. J. J. Lalor, vol. 2, p. 126.

it as a case of "double coincidence"; for market dealings based on barter imply not merely double but multiple coincidence. The industrial problems of barter would be of a different order but not in any way less serious than those of money economy. For changes in fashions, habits and customs on the one side and inventions, discoveries and technical economies on the other, are bound as often to upset the productional equilibrium. The guarantee for rehabilitating the disrupted equilibrium does not, however, obtain, in view of the attribute of friction that attaches to barter. All the other factors of disturbance, *viz.*, psychology, natural and physical phenomena, discoveries and so forth, are present. The monetary factor is the only one that implies some sort of control; in its absence the transitional difficulties for each age would be far more serious. It is, therefore, possible to over-emphasise the importance of the theory of barter in monetary economics. It is possible to forget that what we are primarily concerned with is money itself and its mysterious interactions on the processes of production, distribution and consumption.

The relevance of barter theory to our discussion has been suggested to arise in several ways. It is often supposed, for example, that what would happen in a barter economy in which lending was done in terms of goods, has some bearing on what should happen under a monetary system. This is true to a limited extent only. Thus, lending in terms of a single commodity, (say) wheat, would mean only that wheat was enthroned as the standard of value instead of some other material. Fluctuations in the production of wheat, for any reason, such as, for example, a bumper crop, would bring so much new purchasing power into the market that the wheat rate of interest in such circumstances could not be relied on to stabilise production all round. Making another assumption, *viz.*, that all the commodities and products are simultaneously accepted in payment of debts, the instability of the production of some of them will cause serious instability in that of others. Whether this would mean average stability for industry as a whole, it is difficult to say. But we may accept the

thesis advanced by Dr. Hayek in his *Monetary Theory and Trade Cycle*, that in the barter system, the rate of interest regulates the rate of investment in such manner that only those investments can be carried out which are justified by the available real savings and that this avoids the disparity between savings and investments, resulting from our adherence to money.¹ It is not necessary, however, to fall into the error that in barter the correspondence between saving and investment would come about in any direct manner, that all saving would necessarily be of the form of investment goods, which would themselves be the investment desired. Some investments might indeed take this direct form. But a greater mass of saving would be by way of accumulation of consumption goods. This would mean that the relative valuation of consumption and investment goods should be in favour of the latter, and that, therefore, the social productive powers would be automatically turned into those channels of investment, where they would get a better reward. Thus, under barter no saving could possibly be "abortive." Nor, on the other hand, could saving be "forced" on society, by production of more investment goods (say, by means of a planned economy), for this would be punished and discouraged by low relative values for investment goods.

§ 4. The above reasoning seems to suggest that barter is a good model for monetary management in so far as it solves the problem of the savings-investments disparity. That impression must, however, be corrected. For, although barter in itself provides a remedy for an automatic solution of that problem, it does not help us to ascertain the *ideal* rate of interest that should be adopted under the *money* system. It is impossible to find out even a weighted average of the several "barter" rates of interest for the simple reason that a stable common measure of these cannot be obtained. Moreover, even if we succeed in constructing such an average "barter" rate, we have no reason to

¹ It is not correct, however, to say that under barter there will be one rate of interest; there will be as many rates as there are loanable commodities. Cf. the slightly different concept of Mr. Sraffa's "commodity" rates *supra*.

suppose that that rate, *because it secured the savings-investment equalisation, under barter, would do so here also*; for other conditions of barter-like automatism do not obtain under the monetary system. It is for this reason, among others, that we have to reject the very ingenious concept of the "natural" rate as being the rate proper to a non-monetary economy.

It remains to discuss some minor points in this connection. The postulates of barter and those of stable-money economy are not the same but entirely distinct. If by stable money we mean that the value of money is kept stable in terms of commodities, then clearly stable money implies an interference—whether good or bad, we are not concerned here to discuss—with the automatic working of natural forces, and differs from barter in substance as well as form. For, in barter, there is no such external interference and production is controlled by intrinsic forces only. Consequently, the rate of interest that would emerge under stable money, in which prices were either rigidly or relatively stable,¹ and production would be advanced or retarded beyond its automatic level, is not the same as the average "barter" rate of interest. Both barter and stable money, again, must be distinguished from Dr. Hayek's "neutral" money, in which the total "quantity" of money is supposed to be kept perpetually constant and neutral towards prices and production. A rate of interest which would achieve this last ideal, again, is not the same thing as either the "barter"² rate or the "stable-money" rate.

In recent times, owing to the collapse of commodity prices and partial failure of the price-mechanism to re-assert a world equilibrium of production, some international dealings of the type of barter have taken place. This has started speculation as to the future of the money economy, and it is said that a return to barter would be the next stage in economic evolution. Whether this would or should be so it is not for me here to say. There is no need, how-

¹ Cf. Professor Cassel's "true" rate, Chap. III.

² Still Dr. Hayek in his *Prices and Production* seems to treat them as practically identical.

ever, to suppose that a return to barter would mean the elimination of all the economic problems arising from changes in relative valuations. True, money sometimes distorts the vision and puts false appearances on the realities of economic life and thus necessitates our probing deeper, viewing kaleidoscopically what is happening in the realm of realities.¹ But this is not the same thing as to visualise the modern money economy as a mere disfigured replica of its cruder ancestor.

¹ This is what I would call "real economics," the subject-matter of welfare economics. Real economics must not be confused with barter; the former is a viewpoint, the latter a system. For example, a study of the "real ratio of interchange," or of the "effort value" of money, or of "real wages," or of "real interest" would be an exercise in real economics and is, in each case, *relevant only to the money economy*.

CHAPTER XII

THE NATURE OF CAPITAL

§ 1. IT is not possible here to go even superficially into the theories of the nature of saving and capital, propounded from the beginnings of economic science. These are so numerous and the differences between them are so keen that any attempt to synthesise them must fail for the simple reason that truth does not always lie in compromises. The simplest method would be, therefore, to state one's own views on the subject somewhat in a definitive manner. Before doing so, it will be useful to direct the reader's attention to certain important distinctions underlying the concept of saving.

The first and most obvious approach to the theory of saving is the monetary approach. This is the only possible way in which large-scale social saving can take place under a monetary economy with its implication of division of labour. The importance of this approach will be clear in the following pages. The second and less obvious approach, but which has perhaps claimed more attention than is due to it, is the "real goods" approach. Herein either the saving is supposed to be done in the form of goods themselves, as, *e.g.*, under barter, or if it is a money economy, inquiry is made as to what capital and consumption goods are saved "up" for the proposed investment. I have already referred in previous sections to the fallacy of regarding that social saving as a whole can, under money, take the form of actual goods. Of course, real capital will be produced but that will be the result of investment, the aftermath of saving, but saving itself cannot take the shape of saving "up" of real goods.¹ "Saving," as Robertson so brilliantly puts it, "is the one thing which cannot be saved."²

¹ Cf. Prof. Cannan, Note on "Saving and Usury," in the *Economic Journal*, March 1932, for this notion of saving.

² *Economic Journal*, September 1931, p. 410.

Another and slightly different version of this approach to saving is that which regards society as virtually saving "up" consumption goods for the maintenance of that section of the productive society which is engaged in the production of new investment goods. This is the notion underlying the classical concepts of the "wages fund," the "circulating fund," and the "revolving fund," appropriately disguised in each case.¹ It can be shown, however, that there is no necessary connection between the consumable goods thus supposed to be saved up and the formation of new real capital. True, the producers of the new real capital must subsist during the time that they are producing it, but if they choose to work by living on, say, only half of those consumable goods which they could legitimately consume, they could still subsist and still produce that real capital. Besides, anyhow even if that section of producers had produced consumption goods, still they would have had to be fed and clothed. The fact is that what the capital-producing wage-earners do with their wages is an entirely extraneous consideration which has no connection whatever with capital-building. What the society *consumes at the moment* cannot be capital: *that to consume which it is prepared to wait is capital.*² The third approach to saving provides a complete answer to this rather confusing issue. This approach is that of the release of the social productive powers which monetary saving facilitates, but which may not be necessarily contemplated by it. This is the ultimate aspect of capital-building. The productive powers of the community are limited; a unit of these cannot simultaneously be used for the production of consumption

¹ Even some modern writers still adhere to this; cf., e.g., Åkerman, *Economic Progress and Economic Crises*, pp. 29-30. Cassel, *Theory of Social Economy* (1932), pp. 36-9, contradicts this classical view. Robertson, *Banking Policy and the Price-Level*, pp. 41-5, seems to hold a somewhat ambiguous position, regarding "finished goods" as only a part, but not an important part of his "circulating capital." This does not, however, answer Cassel's objection.

² The classical "circulating capital" must not be confused with the category "liquid capital" (of Keynes). This too partly consists of finished goods, but they have no relation to the process of capital formation but are capital *in their own right* and not because they aid capital production.

as well as for that of capital goods. By refraining from the exercise of purchasing power on a part of the consumption goods, producible by these productive powers, the individual saver *facilitates* a future *release* of a section of these productive powers for their utilisation in real capital production. True, there is a time-lag between the two processes: for perhaps the consumption goods thus not purchased have been already produced, but if we take into consideration the fact that all these various processes are *continuous* in time, the time-lag will appear to be of no consequence. Thus, under the monetary system, the saving of general purchasing power, which is money, facilitates the release of productive powers. The fact, that this release of productive powers is not fully utilised, accounts for the whole disparity between savings and investment. It is unnecessary to enlarge here upon the hindrances in the way of such utilisation, *viz.*, the failure of the banking system to step in as the "balancing factor" and the disconnected course of the three processes of saving, lending, and investing.

§ 2. Reference has already been made to the fact that Böhm-Bawerk also conceived of the release of productive powers in his Robinson Crusoe example, in which Crusoe is supposed to save not berries but a part of his productive powers for the making of bow and arrows. Böhm-Bawerk has written a brilliant chapter on this concept of capital-formation.¹ Böhm-Bawerk, however, held that "the productive powers, and not the goods which they produce, constitute the *immediate object* of saving." We have seen that the individual saver has no such object necessarily in view; he does not *intend* but *facilitates* the use of productive

¹ *Op. cit.* pp. 101-18. The usefulness of this third approach to saving may be demonstrated by reference to the theory of foreign trade. What happens, *e.g.*, when country *A* borrows from country *B* a sum of money, is that in effect *A* borrows so much productive power. Whether the result of the borrowing will be for capital goods to flow out from *B* to *A*, or whether it shall be consumption goods so to flow out, will depend on the behaviour of *A*. *A* may either import consumption goods only, and use an equal quantity of productive powers (thus released) at home for capital-building, or may import capital goods and allow its own productive powers to go on producing consumption goods. In either case, the result will be nearly the same.

powers in a particular way rather than in another. Apart from this oversight on the part of Böhm-Bawerk, it must be noted, he made no further use of the above important concept of saving. For like most other continental economists, he believed that the release of productive powers facilitated by monetary saving would always be fully directed to capital production. For him and his followers, not the disparity between the *possibility* and the *actuality*—between saving and investment—but the facts of the lengthening and shortening of the social production-period and of over-capitalisation and subsequent “freezing up” of capital constitute the basic principles in trade cycle theory.

Attention of the reader is directed particularly to the first and third of the above three approaches to saving. Saving of money is the immediate fact; saving of productive powers the ultimate one. And here we must go further and make distinction, firstly, between past saving and current saving; secondly, between existing real capital, which is a factor of production, and the addition which is a part of the total output. Failure clearly to distinguish between these various things has been, in my opinion, at the root of the current disagreement over the theory of capital.

§ 3. The past history of saving and lending and of capital production may be a matter of importance when we are considering the wealth and wealth-producing capacity of a community. But in the theory of interest-prices relationship, as in that of short-period fluctuations, what *has been* lacks real significance. How far in the past the processes of saving, lending and investment have diverged from one another is a question which affects us only indirectly to-day. The jarring effects of the disturbances set up in the distant past have long since been harmonised into silence; they have left no rumbling echo behind. The magnitude of the community's past saving and past investment in real capital, perhaps, proportionately affects the current output (directly) and the marginal yield of real capital (inversely). Moreover, the magnitude of past lending, in so far as it has any contractual status to-day, has a bearing on the debtor-creditor

relation. But those effects are of the second order of smalls. For the rest, neither their size nor their kind is of much relevance to the pageant of current and on-coming events. Besides, the present total capital wealth of the community does not exactly correspond to the total amount of social saving done from time immemorial. In cases, saving has been "abortive"; in others, it has been exceeded by investment; in cases, the "hen-roosts" of sinking funds have been "raided"; in others, contractual payments have been defaulted and thus the claims of the original savers on the wealth of productive society have been nullified. There is no record of how much capital wealth of the world has been frittered away by failure to provide for replacement and how much short-term loan capital and released long-term loan capital have been absorbed in current consumption. Above all, there is no knowledge of how much old real capital has been rendered valueless by scientific progress and the depreciating effects of new inventions. There is, therefore, nothing like correspondence between past saving and present real capital, much less between past "real savings" and current bank money. With the ghosts of old saving, lending and investment, we are only secondarily concerned. Our immediate concern is with what is happening at the present moment and what is going to happen in the next.

The foregoing remarks must not be misunderstood to mean that the "tradition or social memory," that is real capital, does not at all influence the moving pageant. In the first place, it has a direct bearing on the saving capacity of the community as a whole; for, the larger the capital wealth, the larger *pro tanto* will be the total consumable and savable income. Secondly, the larger the capital wealth, the larger probably will be the floating fund that is dedicated to replacement; and as this at any time constitutes a substantial part of the available supply of money capital in the market, the level of the market rate of interest will be partly determined by it. Thirdly, the marginal yield of existing capital will be lower (other things equal, *i.e.*, in the absence of new inventions, etc.), the larger is the

capital investment, and, therefore, the demand for capital-disposal less keen. This will affect the normal rate of interest from the side of demand. Thus it must be conceded that the "social memory" is indeed of some significance. Its value, however, in monetary dynamics is, as said above, of the second order.

§ 4. The third distinction, referred to in a previous paragraph, *viz.*, between real capital, the factor of production, and the new real capital, which is a product, remains to be explained. We may define real capital as that part of the embodied results (not necessarily *material*) of the co-operation of the productive agents, which is not meant to be immediately consumed and/or is not in an immediately consumable state. In the beginning, presumably there was no real capital, but only land, labour and enterprise. Then it slowly accumulated, a side-stream of production running into a lake; this accumulated capital joined forces ever since its inception with the other factors of production, the side-stream growing all along and replenishing the "lake" over and above its wastage. The "lake" is thus the factor-of-production aspect, the side-stream represents the new addition. Thus simplified, the distinction looks quite easy. But the confusion really centres round the working-capital portion of real capital. For, working capital appears to be at once both product as well as the factor of production. It is clear, however, that working capital is only a vehicle of imperfect values; and as for these latter society has to wait, for howsoever short a time, the working-capital fund for the time being must remain identified with them; that, however, does not disprove its status as "factor" capital.

§ 5. Let us now proceed to certain institutional facts connected with the process of capital-building. In a footnote in Chapter I, I have already distinguished between (a) saving, the monetary "act" of refraining from spending money income on consumption goods, (b) lending, also a monetary act of making loans of money to investors and consumers—an act which behaves as a buffer between savings and investments and distorts their relationship, and (c) investing, the monetary act, again, of purchasing invest-

ment goods (the *value* aspect) or of paying to make those investment goods (the *cost* aspect). Corresponding to these three processes we have the savings, loans and investments, which are the respective results of the above acts; and again, we have the three systems—the saving system, the lending system and the investing system. The first system includes individual savers and corporations and governments, acting in their behalf. The second includes individual lenders, corporate lenders and self-financiers and the banking system. The third comprises the investor-entreprisers, individual, corporate and governmental. The three functions may and often do in practice overlap, but theoretically they are distinct. The facts, that they do not overlap completely and that there is much division of labour, account, as we shall presently see, for much of the causation of monetary disturbances.

§ 6. Another point requiring our attention is the part played by distress borrowing (by governments and bankrupts) and by consumptional borrowing (by prodigals and governments again). The financial functions of governments put them simultaneously into the rôle of entrepreneurs and that of consumers-by-proxy. Theoretically, these are different, but, practically, the dividing line may be hard to draw. Now, in the former rôle, *viz.*, that of enterprisers, they borrow both for purposes of investment as well as to finance losses, just like any other enterpriser. In so far as they do this they may be said to create demand for *social* savings. In so far as, however, they and the prodigals draw upon the market purely for purposes of consumption, their demand for savings has to be deducted from the total of *individual* savings, and, from the viewpoint of the trade cycle theory, it is null. The reason is that we are, in this theory, concerned with *social* savings and *social* investments. If *A* saves £1000, and *B* borrows £500 from him for real investment, £100 to finance losses and £400 for private consumption, social saving for our purposes is only £600, investment is £500, although total individual saving is £1000. This distinction between social and individual savings will be useful when we come to consider what should be the

level of the complex known as the market rate, which is the weighted average rate ruling in the market for loans of all kinds, whether long or short, whether borrowed for consumption, or for investment or to finance losses.

Thus it is not often clear in current discussions that the pure quantities of social savings, social investments and social income are not on the same footing, either theoretically or statistically, with total individual savings and total individual income. *Much less are the total savings deposits in the banking system any index of the magnitude of either the one or the other.* Neither the total individual savings nor the total social savings, saved from time immemorial, have any connection with the savings deposits in the banks. These former have been already utilised and embodied in the real capital and are not now available in fluid form. Only the Replacement Fund is free. This with the current individual savings forms the total available savings, at any time, whether within or without the banks. The so-called savings deposits in the banks, however, consist not only of the savings consigned to the banks, against which genuine loans have been made, and of part of the Replacement Fund, but also of the *created* loans.¹

§ 7. Before concluding this chapter, we shall briefly dwell upon that very thorny question of replacement of real capital. The fact has to be reckoned with that before computing the social income, we have to deduct from the gross money income, on the one hand, and from the gross new real capital, on the other, a certain proportion for replacement of wastage of capital. Now although in the matter of this replacement, we individually often connive at our own lapses, there is no doubt that on the average and from the social viewpoint, replacement has to be made not in any specific shape of reproducing identical capital

¹ In view of this, Mr. Robertson's remark (*Economic Essays and Addresses*, p. 98) that the "real value of a country's bank money is the same thing as the amount of real savings which the public has put in the past at the disposal of industry, through the medium of the banks"—appears to need correction even if we suppose that the banking system is co-extensive with the entire lending system, and all savings are consigned to it. A similar fallacy is patent in some of the writings of the Austrian and Neo-Wicksellian schools.

goods but at least of producing an equivalent amount of real capital. Here, however, difficulty arises as to what constitutes an *equivalent* amount. The difficulty is formidable especially when we are considering a dynamic economy with a growing productivity. Is the replacement, *e.g.*, to correspond to the money value (say £10,000) or to the actual real capital goods? Pigou has discussed the issue in his Chapter on "Maintaining Capital Intact" in the *Economics of Welfare*.¹ His solution is that it is convenient in this case "to go behind the physical elements embodied in the stock of capital to the real (labour) cost, and say that depreciation is made good by the provision of £10,000 *worth* of new machines." It may be well to adhere to this solution. For one thing, there is no sense in believing that in a rapidly changing structure of industry we do in fact ever reproduce identical real capital goods to replace the old; for another, the whole of the money fund released for replacement must be fully absorbed for that purpose. If that is not so, it will be permanently mixed up with the current social saving which we want to isolate in order to equalise it with current investment.

¹ 1929 edition, p. 47.

CHAPTER XIII

THE TRUE CONCEPT OF THE NATURAL RATE

§ 1. THE question now remains to be decided as to what is the "natural" or "ideal" rate of interest, which forms the focal issue in our discussion of the correct monetary policy. Assuming for the moment that our desideratum is short-period industrial stability, we may say, firstly, that the natural rate we need to investigate is a rate relevant to the short period. This does not mean that it is a short-term rate. The short term of monetary lending and the short period of economic theory are entirely distinct. Nor does it mean that the natural rate is a long-term rate. It is, as we shall see, an *average* concept. Secondly, it is a money rate. By this it is not meant that the rate itself is money, which would be indeed wrong (because the rate is a ratio); but that *the rate is one proper to monetary dealings and not, in any sense, a rate which can or need be expressed in terms of goods or as being proper to barter*. The expression of natural rate in terms of goods is possible only when we have succeeded in tackling the problem of the value of money, and even then it may not be worth our while. This natural rate, then, which is a money rate, can be defined as the rate which equalises the supply of and demand for current *social* money savings, as distinct from either individual money savings or market loans.¹ This rate indicates, moreover, the *ideal* position for the market rate at which it will *maintain* an existing equilibrium. It is, as I understand it, *not* the rate which would reassert equilibrium *once there had been serious deviations from it*.

The supply of social money savings consists of s , the Replacement Fund, *plus* S , the rate of social savings. The demand for social savings consists of i , the demand due to

¹ The rate that would be obtained by the free interplay of demand for and supply of individual money loans, in the absence of the banks' control, would be the "automatic" market rate. This is not as is commonly supposed the natural rate; (such a rate) would not stabilise production.

replacement investment, and I , the demand for investment. We have decided, in the last chapter, to regard s and i as equal. So our definition comes to be very much the same as Mr. Keynes's, which regards the "natural" rate as the rate which makes simply S and I equal. We must remember that s consists of the Replacement Fund for fixed as well as working and liquid capital. The three quantities s , i , and S are more or less stable in the short run; the quantity I , on the other hand, is liable to violent fluctuations. It is this that needs to be properly brought under control.¹

Thus, to repeat our definition, the natural rate is that money rate which equalises current social savings with current social investments (including replacements and new investments), or, what is almost the same thing, equalises the rate of saving with the rate of investment, irrespective of the rate arrived at by the free action of the forces of supply of and demand for available loanable funds² (which would be the "automatic" market rate). Circumstances are conceivable under which the latter may be fairly high but the market rate will have to be so low as to satisfy the genuine demand for investment and over and above that to satisfy *à outrance* the demand due to losses or prodigal expenditure. Thus, the natural rate is only the ideal position of the market rate to which adherence on the part of the banking system is essential if equilibrium is to be maintained. *The free interplay of economic forces cannot be relied upon to establish this rate.* For there are numerous factors making such a coincidence impossible over any length of time. In the first place, there is divergence between social savings and gross total individual savings. Secondly, the loans, which (and not savings) are the thing lent out, could seldom coincide with either total individual or

¹ Quantities s , i , I and S refer to a unit of time of substantial length; otherwise, it would be impossible continuously to achieve equilibria of the two quantities, S and I .

² I have to warn the reader, again, to remember that current social saving consists of (i) Replacement Fund, pertaining to the time-unit, and (ii) the rate of social saving during the time-unit. This has *nothing to do with the savings deposits in the banks*, and, as I have already said, it is a wrong view which merely requires complete absorption in investment of all savings consigned to the banks, in the past or at any time.

with social savings. Thirdly, a vast quantity of loans are absorbed in consumption and industrial losses.

For these reasons, the natural rate is not a readily ascertainable rate, and *is more abstract than is generally believed*. As a "conceptual" rate, however, its theoretical value is great. If the market rate were kept at the same level as the natural rate, we should expect, by definition, that *S* and *I* (in Keynes's terminology) would be equal, and the problem of short-period fluctuations, due to divergence between costs and prices, would be solved.

§ 2. The market rate has been described by Keynes as a "complex" of short and long rates ruling in the market. To call it a mere complex, however, would be rather vague, in view of the fact that short and long rates diverge widely at times. There is, indeed, a short-rate "complex" as there is a long-rate one; because the rates which these complexes comprise often cluster together round a nucleus as it were. But when the short-rates are in the neighbourhood of, say, 1 per cent and the long rates in that of, say, 5 per cent (while the natural rate is believed to be near, say, 3 per cent), we cannot adequately describe the short and long rates as forming one market-rate complex. The better explanation would, perhaps, be to regard it as a weighted average of the various short and long rates prevailing in the market, weightage being ascertained by reference to the size of the particular "reservoir" to which the rate belongs. The weight will itself be a shifting one, inasmuch as the relative magnitudes of the "reservoirs" of short and long funds alter according as the bank rate is raised or lowered, or owing to other causes. But a rough notion may be had as to the relative magnitude and the shift would not be considerable over short periods. What is needed is that the principal reservoirs of long and short funds are replenished or depleted as the case may be and investment in fixed, working and liquid capital quantitatively controlled. It is the function of the banking system to inflate or deflate these reservoirs, as well as to observe a certain proportion between their relative magnitudes.¹ The power of the banks

¹ Cf. Robertson, *Banking Policy and the Price-Level*, p. 91.

directly to influence the size of the long reservoir is limited under the individualistic system by prudential considerations. Even within the four corners of the system, however, it might be open to banks to undertake direct long-term financing and thus help to enlarge the long-term reservoir. Without going far astray from financial rectitude, on the other hand, they can exercise their influence on it through the medium of the short reservoir by means of bank-rate manipulations.¹ Apart from this quantitative control, the banking system is always in a position in view of its intimate relations with industry to exercise a qualitative control over investment. It can influence the course of relative prices in a particular industry or group of industries by methods of discrimination especially among the "unsatisfied fringe of borrowers." Thus, simultaneously with the attainment of general or average stability, relative stability also can be achieved.

§ 3. As pointed out more than once, in the foregoing pages, the natural rate is not the same thing as the marginal yield on past investments in real capital; nor is it identical with the prospective yield on future investments which at times may considerably differ from the marginal yield on existing capital. Several writers have unfortunately mixed up not only the natural rate with the marginal yield but the latter with the yield on future investments. Now the yield on future investments is "expected," not "realised," and, therefore, depends very much on the state of the investor's psychology and on the availability of new inventions and possibilities of new demands created by changes in tastes and fashions. The marginal yield on past investments is thus apparently less important to current calculations of investors than the expected yield on future investments.

Wicksell held the view that the natural rate represented an exact coincidence between the demand for loan capital and the supply of savings and more or less corresponded to the prospective yield.² No such synthesis is, however,

¹ This matter will be discussed in Chap. XIV.

² Similar views are held by Prof. F. Knight, "Some Neglected Factors in Normal Interest," in the *Quarterly Journal of Economics*, 1915-16, pp. 279 ff.; and Akerman, *Economic Progress and Economic Crises*, and *Some Lessons of the World Depression*. Also by Hayek and some of the Austrians,

possible ; because the prospective yield is only a consideration on the side of demand in determining the natural rate. True, the industrialist is influenced by the disparity between the market rate and the prospective yield. It is also admittedly the mainspring of the kind of activity underlying industrial change. But the prospective yield cannot be made the norm or criterion for market-rate manipulation for the simple reason that it involves circular reasoning. The marginal yield, as well as the prospective yield, depends to a great extent on how much investment has already taken place ; this, again, in its turn, depends on the position of the market rate relative to the previous prospective yields. Besides, *the prospective yield is not such a definite thing as many people are apt to think ; opportunities for new investment exist almost in limitless abundance.* There are certain projects which can be profitably undertaken at 5 per cent ; others only at 4 per cent ; still others at 3 per cent and so on. Whether they are practical propositions depends on the position of the market rate itself. The total volume of new investments is thus controllable by the market rate remaining at a certain level. *It is the function of the market rate to keep the total of the new investments at the level indicated by the new social savings and not to enter into a kind of sliding race with the prospective yield or yields.* The position of the market rate, at which the inelastic process of social saving and the elastic one of social investing are kept *pari passu* equal, is the *ideal* position, which we call the " natural " rate.

§ 4. Whether the volume of new investment *per se* is great or small is a matter of small consequence, so long as the balance between new social savings and new real investments is observed. It is the fact itself of the disparity between these that is largely responsible for a rise or fall in prices by consequent expansion or contraction of production. It is a theory widely held that the excessive capital production in the upward phase of the trade cycle results in a subsequent plethora of consumption goods and thus generates the cause of its own undoing. This is true to a limited extent only ; *e.g.*, in so far as more working capital is invested in, for the production of consumption goods, the volume of these will

increase by a certain proportion. The total addition, however, to the capital stock of the community, during the upward phase of the cycle, must be a *relatively small proportion* of the latter. The *secondary* results of this addition on production as a whole, whether on consumption or investment goods, in the secondary stages, are not likely to be of such a magnitude as in themselves to constitute the subsequent fluctuations in production. *It is the fact itself of capital production exceeding or falling short of the optimum indicated by social savings* that is the principal event in the trade cycle, as this causes fluctuations in the industrial activity of productive forces.¹ Continuous moving equilibrium between

¹ Two distinguished contributors of the *Essays in Honour of Gustav Cassel*, J. W. Angell and J. Åkerman, *e.g.*, hold the view that it is changes in the absolute volume of savings-investments or, to use a neutral term, of accumulation, that are more important to trade fluctuations than mere disparities between savings and investments. Åkerman has given an ingenious formula for finding out the "savings quota" of the national income and believes that if this is kept steady, all would be well. This view is held by a number of other writers also. However, the obvious point against this theory is that changes in the absolute volumes of savings and investments are not capable of causing increased or diminished activity but only of diverting activity more into the channels of capital production than into those of consumptional production.

On the other hand, the theory that alternate investment activity and subsequent increased activity in consumption trades accounts for the rise and fall of the cycle, is not borne out by statistics. Cf., for example, the following :

INDICES OF PRODUCTION OF PRODUCERS' AND CONSUMERS' GOODS
(Average, 1925-29 = 100) P—Producers' goods
C—Consumption goods

Country	1925	1926	1927	1928	1929	1930	1931	1932	1933
<i>Germany :</i>									
P	88	81	111	108	112	91	70	54	63
C	87	89	111	109	104	101	94	85	93
<i>France :</i>									
P	88	100	88	106	118	118	102	72	81
C	93	104	92	105	106	100	91	75	93
<i>U.K. :</i>									
P	91	—	102	100	107	96	78	75	84
C	99	—	101	100	100	90	88	90	94
<i>U.S.A. :</i>									
P	97	100	91	101	111	80	52	29	43
C	95	97	101	101	106	90	90	81	91

(From *World Production and Prices, 1925-33*, League of Nations.)

The above statistics would show that the activity in productional and consumptional trades coincides very remarkably and that there is no alternate disposition of the two types of activity.

I and *S* will secure costs-prices equilibrium and, thus, industrial stability. This view of the matter is a correct therapeutics of the trade cycle.

Another caution may be made in connection with the natural rate which I have described as a short-period consideration. It is a popular misconception that it is the bank rate which needs to be equalised with the natural rate. But the bank rate, which is only a "control" rate of the short-term variety, cannot be assimilated to the natural rate. In the first place, it is seldom the ruling rate in the market in which the variety of short and long rates ruling is almost bewildering. Secondly, even granting its undoubted power to force up or down the short-rate complex in the first instance and the long-rate complex afterwards and indirectly, it is only the average of short rates over a series of short periods that comes anywhere in the vicinity of the long-rate complex and the natural rate. It is not necessary, therefore, to move the *bank rate* to the position of the natural rate. What is necessary and sufficient is that it should be kept at such low or high level as to make the weighted average of the market rate (complex of short and long rates) equal to the natural rate. If there is evidence of prices beginning to fall below costs, *e.g.*, the short rate, which will be necessary to call forth enough investment to balance savings, will have to be much lower than the long and the natural rate in those circumstances. A short rate that will transfer a superabundance of funds from the short to the long reservoir of loanable funds will have to be much lower (say, 1 per cent) than the long rate need be (say, 4 per cent) to call forth enough investment. Conversely, in a period of boom-like tendency the short-rate complex, lorded over by the bank rate, will have to be considerably above the long rate, in order that the weighted average of the market rate is equalised with the natural rate.

§ 5. The whole of the foregoing discussion relates to a dynamic state in which the endeavour of the banking system is assumed to be the maintenance of *a constant and moving equilibrium* of savings and investments and of costs and

prices. As has been repeated by practically every modern monetary economist, the banking system has an easier task to perform in maintaining the keel of the industrial ship even on an equilibrium *once established*, by means of mere minor adjustments in the price or interest-rate mechanism, than in re-establishing the balance of the ship *after it has completely lost it* and has begun to rock heavily. In every trade cycle in history the ship has been sooner or later saved from complete capsizing and *bouleversement* by some fortuitous circumstance or other, coming in as a *deus ex machina*. Often the banking system itself has blundered into the right course of action and, somehow or other, found itself mastering the situation again. There is plenty of evidence in monetary and industrial history for this kind of happenings of the order of *disequilibrium* economics, and, perhaps, none at all for the economics of *moving equilibria*, as postulated in the foregoing discussion. I believe, rightly or wrongly—and in this, perhaps, some kindred spirit might corroborate what I say—that in monetary theory we have to deal precisely with these two varieties of economic postulates and that the analyses appropriate to the two are entirely distinct and distinguishable. *The kind and extent of monetary control, which would be necessary under conditions of existing equilibrium, with a view to its further maintenance are not the same as are called for under conditions of a serious disorder of practically every phase of economic life.* The latter situation, like all desperate situations, calls for desperate remedies. Not a mere maintenance of the market rate at an imaginary level, demonstrated by the natural rate, but more positive measures, coaxings, boostings, propaganda and active stimulants, such as a progressive public works policy, would be necessary under such circumstances. For these reasons, I firmly believe that the concept of the natural rate, which is only appropriate to the theory of moving equilibria, cannot be extended to that of a disequilibrant economy. The natural rate, which is thus proper to the theory of moving equilibria, is a fairly stable rate and emerges when an equilibrium has been reached in the relations between savings and investments and between costs and prices.

Moreover, it solves the problem of what is to be done next, when once such equilibrium is established. Being a concept which belongs neither to the static state nor to the dynamic, present-day world, but to an idealistic state of moving equilibria within the four corners of monetary dynamics, it does not provide any readily available formulæ for practical banking policy.¹

§ 6. This may appear a counsel of despair for banking theory, but we have to face the fact boldly that bank-rate policy by itself cannot achieve miracles. In this connection, I am inclined to attach some weight to the criticism of those who have no faith in the ability of the banking system by itself to control extreme fluctuations in trade and employment. But, I do not, on the other hand, share the views of those pessimists who think that banks can play no part or only a passive part in the movement for the rehabilitation

¹ Hence, I do not find myself in agreement with Keynes, who says (*Treatise*, vol. 1, p. 159) that control of investment may not always be "practicable over short periods, *since for short periods the natural rate may sometimes fluctuate in an extreme degree*" (italics mine). Keynes, in this passage, as elsewhere, seems to suggest that the natural rate fluctuates so widely over short periods, that the banking system will have to play for very high stakes in order to bring the monetary machine under its control. This, however, is, to my mind, an unnecessary and illegitimate extension of the natural rate concept, because a rate which would maintain equilibrium once it is established, cannot be the same as the one which would be required to make the unwilling horses drink. The former rate is *the* natural rate; the latter, which is a much less stable consideration, is neither the natural, nor normal, nor equilibrium rate, but only an "opportunistic" rate—a compelling force in the monetary machinery, a whiff of oxygen administered to a moribund organism, or a dose of morphia administered to a hyper-active one.

Another reason why I cannot accept Keynes's interpretation is that even if (*e.g.*) in a severe depression the market rate is brought to the level of the natural rate and investment is equalised with what little saving is going on (as the rate of saving will have certainly diminished) will not be sufficient to secure full employment; in such circumstances, *investment will have to exceed saving up to the point at which full employment takes place*, and then continue to be equal with saving.

The subject matter of this chapter was written nearly two years ago. I am glad to find corroboration of my theory, that the natural-rate notion appropriate to equilibrium position fails to provide guidance in abnormal times, in Mr. Robertson's recent article on "Industrial Fluctuation and the Natural Rate of Interest," in the *Economic Journal*, December 1934. In this article, Mr. Robertson develops his interesting concept of "quasi-natural" rates of interest. While both of us are investigating conditions of departure from equilibrium, however, I do not believe that there are two or three quasi-natural rates, but that there exists a number of "opportunistic" rates appropriate to given situations of disequilibrium, at various stages.

of a seriously disorganised and topsy-turvy industrial world. The action of the banking system in replenishing the money market with loanable funds, by means of the two mechanisms of the bank rate and open-market operations, supplemented in emergencies with direct dealings in intermediate or long-term investments, by purchases and sales of gilt-edged and industrial securities, cannot by any means be styled "passive." They may be called *actively* passive, if such a paradox be permitted. It is true, *e.g.*, that downward changes in the market rate are merely *invitations* addressed to investors to borrow and may often be disregarded by them; similarly, on occasions, there may be a good deal of "gate-crashing" in more cheerful times. But it must be admitted that if there were brought about drastic changes in both the short-term and long-term conditions of lending, they will become an active and compelling force acting on the imagination of investors, who are ever calculating the position of the market rate relative to that of the prospective yields on a variety of future investments. Thus, it is clear that the banking system wields an undoubted power over the shaping of industrial events, although in periods of serious disequilibrium the banking system cannot by itself bring about a speedy readjustment in the industrial and the monetary situation; because, at such times, there is a great tension of forces set up by psychological influences and by the physical facts of production, which mark the transition to increased capacities of production and re-adjusted values. This tension has to be overcome and the unwilling horses must be coaxed into drinking the water of loanable funds. The disagreement between the ideas of lenders and borrowers, which is a characteristic feature of all depressions and is purely due to unreasoning pessimism on both sides, has to be settled. This function cannot be assigned to the natural rate, because it is not a quantitatively determinate function. Secondly, it is extremely doubtful if the market rate by itself can, at any practicable level, especially in depressions, cause so much investment to be made as to correspond to the savings. The prospective yield, although ordinarily dependent on the marginal yield

on past investments, is largely affected here by psychological factors : it is never at any time an absolute or determinate quantity. It is, therefore, very much liable to circular reasoning and the market rate, which is from time to time compared by investors with the fluctuating prospective yield, cannot be given a definitive value as a natural rate at any time.

CHAPTER XIV

LONG-TERM AND SHORT-TERM RATES

§ 1. It has been already maintained elsewhere ¹ that the market rate cannot be regarded as a "complex" of short and long rates, but as a weighted average of both classes of rates, weightage being according to the magnitudes of the respective reservoirs of loanable funds. This market rate, however, has no connection whatever with the "pure interest" of economic theory. As has been acknowledged by many writers, the "pure interest" of economic theory is an ethereal unreality and has no statistical basis in the actual facts of the business world. Here there are as many rates—short, intermediate, and long—ruling, as there are markets with varying degrees of duration, risk and recoverability involved in them. The factor of risk, moreover, complicates the contracts which are the results of the impact of conflicting ideas entertained by borrowers on one hand and by lenders on the other. The risk factor varies and fluctuates not only with reference to the credit of the borrower but also according to the economic environment, and it will be seen that it is uppermost in periods of serious depressions. That is a main reason why, in bad times, there is a tendency for long-term funds to become liquid and get shifted to the short-term reservoir of floating funds and thus bring the short rate down to ridiculously low levels. The ideas of all lenders in bad times are of the *sauf-que-peut* variety, as each one of them seeks his own safety, by holding funds as liquid as possible.

As demonstrated by Keynes,² in periods of depression the ideas of lenders and borrowers in the long-term market fail to coincide mainly for the reason that "distress" borrowers are very active in such periods. Governments who cannot balance their budgets, collapsing business firms in need of funds to finance losses, long-term debtors needing further

¹ See Chap. XIII.

² *Treatise*, vol. 2, p. 380.

accommodation and many others, who for various reasons closely connected with the slump are brought into the chorus of hapless borrowers, create a demand for long-term funds which is not easily satisfied. The result is that the long rate remains unreasonably high and prevents any borrowing by genuine borrowers for long-term investments. On the other hand, in good times, there is a feeling of buoyancy among the lenders and they discount the element of risk involved in lending long and thus aid the process of investment in fixed capital to a greater extent than is perhaps good for them. Borrowers, too, need funds for long-term as well as short-term investments and get indebted up to the hilt in both markets. The short rates, however, respond more quickly to the demand, and for three reasons. In the first place, the short-term reservoir is usually the smaller and is more mercurial in character ; secondly, there is a transference of funds from it to the long-term reservoir ; and thirdly, the demand for short loans is ever on the up-grade in the later stages of the boom on account of the working-capital needs of industry.

Thus, if any monetary policy, which regards the $I = S$ and costs-prices equilibrium as the means to industrial stability, is to succeed, it is necessary that it should explore the avenues of so controlling the long-term market, which is more important in this connection than the other, as to cause sufficient investment particularly in fixed capital. Now, in the present-day, individualistic society, the function of long-term investment in fixed capital does not customarily fall within the scope of the banking system and, moreover, the orthodox theory of banking does not regard direct control of long-term investments by the banking system as prudent business. The "control" rate of the banking system is a short-term rate. This, supplemented by open-market operations, is indeed capable of exerting an influence on the short-rate complex at any time prevailing in the market. The problem which faces us here, however, is not its ability to affect the short-rate complex but the influence which it is capable of wielding upon the long-rate complex.

§ 2. In his study on *Money Rates and Money Markets in*

the United States, Mr. Riefler, relying upon statistics¹ of short and long rates, arrived at two main conclusions, *viz.*, (a) that short rates were more sensitive than long rates and (b) that there was an equivalence of proportional changes in them; or, in other words, the actual deviations, divided by the standard deviations of each rate from the customary range, gave closely parallel results. Keynes picked up this clue in his *Treatise* and embodied it into his theory of savings and investment, in the form of a proposition that "short-term rates influence long-term rates more than the reader might expect,"² and that, consequently, the banking system had one more weapon at its disposal in controlling the rate of investment.

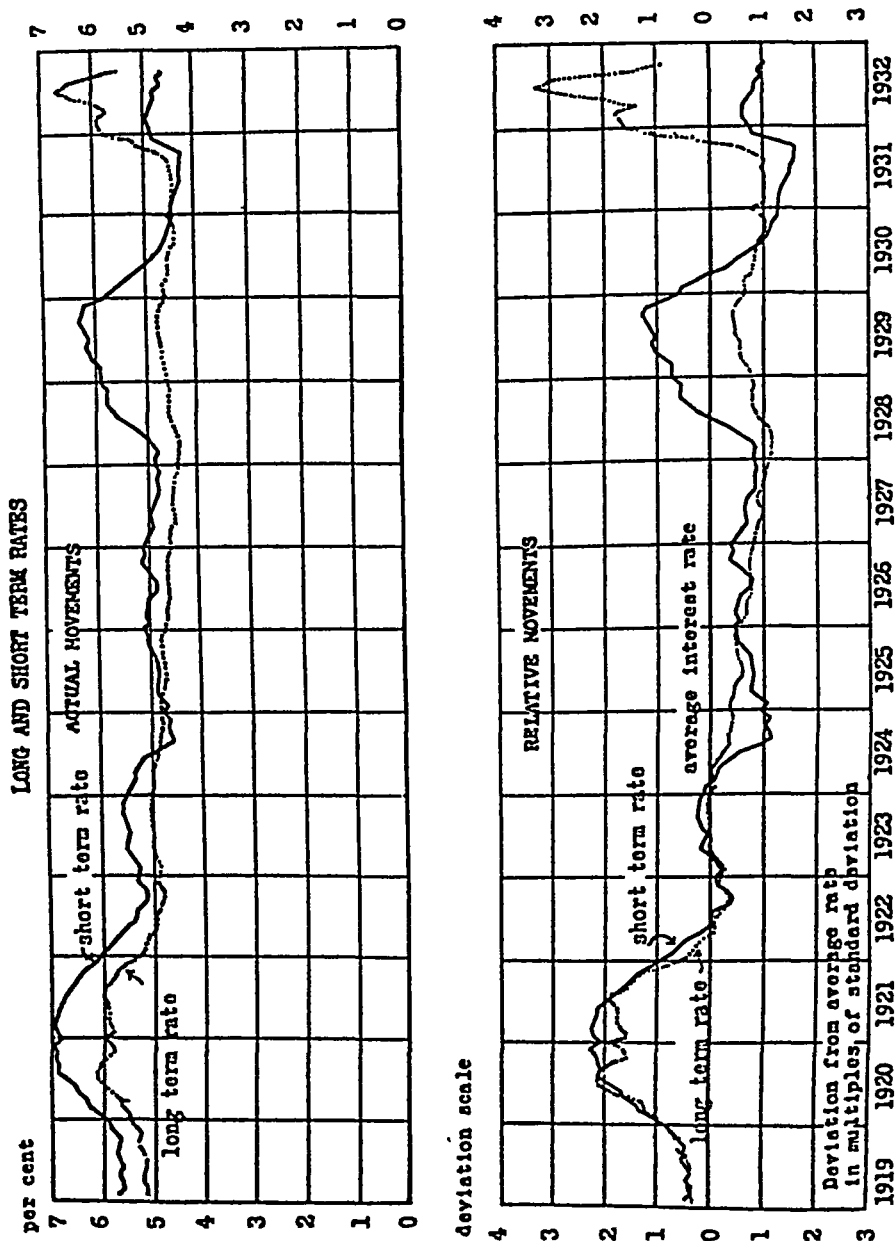
The Riefler-Keynes theory has been assailed by Professor Simmons in the *American Economic Review* (June 1933) on the ground that the equi-proportionality of fluctuations in the short and long rates, which Mr. Riefler derived from statistics for the period 1919-28, is not borne out by subsequent movements of the two rates for the period 1928-32. Professor Simmons, therefore, holds that Keynes's scheme of investment control, based upon Mr. Riefler's figures and conclusions, would not be effective. Keynes, in reply to this, rightly contends that he never advocated the control of the long-term rate of interest "solely by manipulation of the short-term rate of interest." In the following paragraphs, therefore, my purpose is not to defend Keynes's position in so far as it also happens to be mine, but to try to explain why there may be very wide proportional fluctuations in the long rate relative to the short rate on special occasions.

§ 3. Professor Simmons has, indeed, slammed the door of logic by asserting that a theory, which is not true of "abnormal" times,³ cannot be of any use at all; but it appears to

¹ See Chart on the opposite page, which was worked up to the year 1928 by Mr. Riefler and later on extended by Prof. Simmons, for 1928-32.

² *Treatise*, vol. 2, pp. 362 ff.

³ *American Economic Review*, June 1933, "There is little help to be found," he says, "in considering the period from 1928 to 1932 as abnormal and in insisting that in general the relationship does hold. Of what consequence is a theory of control, the mechanism of which does not operate in abnormal periods?" (p. 268).



N.B.—Chart, prepared by Mr. Riefler and later extended by Professor Simmons, showing the relative and actual movements of the short-term rate (being an average of short-term rates prevailing in the principal cities of the United States) and the long-term rate (representing the yields on 60 high-grade American bonds).

Reproduced by permission of the Authors and the "American Economic Review."

me that the explanation of the phenomena lies in this very abnormality, which, *e.g.*, characterised the years 1929-32. To my mind, *independent* influences may and very often do cause wider proportional fluctuations in the long rate and thus either *nullify* or *reinforce* the effects of the deliberate short-rate variations. The working of such influences is particularly noticeable during periods of overwhelming industrial crises or great wars, which destroy the foundations of confidence or cause an enormous destruction of capital wealth and wealth-producing capacity. For instance, the most characteristic feature of the post-War boom and crisis in the United States, as compared with pre-War conditions, was the greater instability of the long rate. The following figures, taken from Miss Karin Kock's *A Study of Interest Rates* (p. 129), shows the spreads between maximum and minimum levels of the lowest monthly long rates computed with reference to the behaviour of high-grade bonds, during three successive crises.

	Maximum.	Minimum.	Spread.
1892-5	5.07	4.35	0.72
1907-9	4.53	3.97	0.56
1919-22	5.66	4.46	1.20

There is no evidence that during these periods there was any determined effort on the part of the monetary authorities to saturate the credit market with the instruments of the "control" rate or open-market operations, either in the United States or elsewhere. In the circumstances, I am inclined to believe that the high correlation between relative movements of short and long rates was rather due to the long-term reservoir exercising a pull on the short-term reservoir than *vice versa*. The result, probably, was that the short rate was seen to move correspondingly in rhythm with the long rate. Even in the period 1929-32, if it were not for a concerted international policy of cheap money, I believe, the short rate would have danced in tune with the long rate as it did (*e.g.*) in the catastrophic period of 1919-22, both rates having shown a high degree of relative deviation from the average rate in the positive direction, as the Chart would show.

§ 4. The conclusion which may be drawn from these facts is as follows. The influence of the short rate on the loanable funds in the long-term reservoir seems to be exaggerated by the generalisations which Mr. Riefler formulated and Keynes has made the basis of his proposals. The fact that the short rates showed the same equi-proportional variability as the long rates does not necessarily prove that the long rates were being affected in a *causal* manner by the short-term credit management. The long-term reservoir of loanable funds is much too large for the short rates to influence its size to the extent suggested. Both Mr. Riefler and Mr. Keynes have given a variety of reasons why the short rate should wield a considerable degree of influence on the long-rate complex.¹ All these reasons certainly explain why control of the short-rate complex may bring about certain corresponding variations in the long-rate complex. But the conclusion at which Mr. Riefler arrived, *viz.*, that "The surprising fact is not that bond yields are relatively stable in comparison with the short-term rates, but rather that they have reflected fluctuations in short-term rates so strikingly and to such a considerable extent," must be interpreted to mean not that the formation of movements of the long-rate complex is necessarily *dependent* upon alterations in short rates,—that there is a causal sequence between the two as from short rates to long rates, but that the movements are broadly parallel, if both rates are left untampered or are interfered with only to a limited extent. During the years 1929–32, the short-rate complex was artificially lowered, but what little influence the short rate could wield on the long rate was not sufficient to cause in the latter equivalent relative deviations and the long rate remained at high levels owing to other causes; for other causes,² such as distress borrowing by Governments and private persons and lack of confidence among lenders with regard to long-term loans, did indeed prevail. These causes have created a tension, which cannot be overcome by merely tinkering with the short

¹ *Treatise*, vol. 2, pp. 352–57; and Riefler, *Money Rates, etc.*, pp. 112 ff.

² These have been very well described by Keynes, *op. cit.*, pp. 378 ff.

rates for a year or two. In order to overcome the psychological tension and to saturate the long-term reservoir with loanable funds, the short rates will have to be held at consistently low levels over a considerable period of time. Such a measure will have, again, to be supplemented with direct long-term financing by purchases of long-term securities through the industrial banks. Only after a general return of confidence should the banking system exercise caution in this connection, for such a return of confidence will imply an agreement between the ideas of borrowers and lenders which would necessitate no further spoon-feeding of industry. While, therefore, it may be conceded that artificial movements in the short-rate complex are not capable of bringing about such considerable fluctuations in the long-rate complex as Mr. Riefler's Chart would suggest, I do not think that the banks are incapable of bringing the long-term market within their control by means of the various instruments at their disposal.¹

¹ I have not with me the entire statistical material necessary to bring the contents of the Chart to date, but on referring to statistics in the latest Federal Reserve Bulletin, bearing on the question of average short rates and high-grade bond yields, I am inclined to believe that the course of short and long rates since 1932 lends greater support to the Riefler-Keynes proposition, and seems to suggest that in less abnormal times the relative movements correspond better though the causal connection is not clear.

CHAPTER XV

THE GIBSON PARADOX

IN this chapter we may now discuss a problem which appears to belong to the long-period relation between interest rates and prices. This has been discussed by Keynes in his *Treatise* under the heading "The Gibson Paradox." Although, in view of Keynes's proposition that a correlation which is proper to the short period is extended to the long period owing to the existence of a long-period coincidence between the market-rate and the natural rate, it appears to be a paradox, there is nothing really paradoxical in the correlation. I have, however, followed him in calling it a Paradox, for the sake of recognition and identification.

§ 1. Keynes's hypotheses have not exhausted the possibilities of explanation, although he appears to be nearer the truth than either Mr. Gibson or Professor Fisher. The facts of the so-called "Gibson Paradox" are briefly these. The correlation, that Mr. Gibson sought to establish, is between the yield of Consols and the wholesale index with adjusted base and its coefficient works out in the neighbourhood of $\cdot 9$ on the average; it is a long-period or intermediate rather than a short-period phenomenon; it is higher (as Mr. Peake has shown) for the long-term rates of interest than for the short-term. It must be conceded that the correlation is not merely a coincidence but an established fact which cannot be explained by reference to the well-known tendency of the short-term rate and prices rising and falling together in the upward and downward phase of the trade cycle. In fact, as Mr. Peake has shown, the correlation coefficient for these short-period movements of prices and the short rates is considerably lower (about $\cdot 6$) than in the case of the long-period movements of prices and interest rates. It may be definitely asserted, however, that there is no need to look upon the natural rate as a *long-period* rate. The

Gibson Paradox is, in my opinion, explainable without any such assumption.

Keynes rightly insists that the long-period relation between interest rates and prices cannot be regarded "as an example of Professor Irving Fisher's well-known theorem of the 'real' rate," although I am unable to follow the reasons which he adduces for this. At best, Fisher's "real" rate or even its modified version may be useful in explaining short-period fluctuations of interest rates and prices. It is *impossible for foresight to be so foreseeing* as to touch the long-period relation of interest rates and prices. To say that it is merely an expression of the natural tendency of the "real" rate towards self-adjustment through positively correlated changes in the money rates relative to price-changes, would be equally fanciful. Fisher's "real" rate has been already shown to be fallacious so far as it makes the "real" rate equivalent to the money rate *plus* or *minus* the appreciation or depreciation in *both* principal and interest. Even to plead, on the other hand, that the new interpretation of the "real" rate, as suggested by me, was the thing that Fisher really implied would be out of place; for the assumption itself is unwarranted that the "real" rate does, in fact, so adjust itself. There is no visible proof for that. The simplest interpretation is, therefore, to regard the sympathetic rise or fall in interest as really an expression of the *passive* aspect of the interest-prices relation, as maintained in the following paragraphs.

On the other hand, the theory that interest is a "certain" price which falls and rises just like any other price is to indulge in *ignoratio elenchi*. The question is not simply why the interest rate falls or rises along with prices, but that it reflects the trend in such close correlation, which cannot be said of any other *single* price. Besides this view of the Gibson Paradox belittles altogether the important rôle that this "certain" price, interest, plays in the movements of credit and of the price-structure as a whole.

§ 2. Keynes's theory is that when "a long-period movement in the natural-rate of interest is in progress, there is a prolonged tendency for investment to fall behind saving

when this rate is falling because the market-rate does not fall equally fast," and *mutatis mutandis*, in the opposite case. "If the market-rate of interest moves in the same direction as the natural-rate of interest but always lags behind it, then the movements of the price-level will tend, even over longish periods, to be in the same direction as the movements of the rate of interest."¹ The most obvious objection to this explanation would seem to be as follows. In his treatment of trade cycle, Keynes suggests that so long as the market rate and the natural rate are divergent, equilibrium cannot be established; for, by definition, the natural rate is the rate at which saving and the value of investment are exactly balanced. Thus if the disparity between the two rates is supposed to go beyond the limits of the short period, what is there that prevents an uninterrupted fall or rise in the unadjusted price-levels? This objection might be brushed aside by saying that it was possible for the market rate to remain (*e.g.*) on the average (over a series of short periods) below the natural rate when the long-period trend of prices was a rising one, although it might at times coincide with it so as to establish temporary equilibria. But the question arises, what ground is there for the belief that the market rate does, in fact, remain above the natural rate in the circumstances? I do not know how far this reasoning would upset Keynes's theory; but it may be permissible to venture an explanation on somewhat different lines.

§ 3. In Chapter I, I have discussed the "passive" aspect of the interest-prices relation, and in a footnote I have already suggested that the Gibson Paradox may be explained by reference to it. From a study of the facts, it appears that in the nineteenth century, although there was some measure of "management" on the part of Central Banks, it was not a very intelligent management and, on the whole, the large supplies of gold, which became available, had forced upon them monetary policies which could hardly be called "active" or of the nature of deliberate control. In the circumstances, it must appear that *the*

¹ *Treatise*, vol. 2, pp. 204, 205.

interest rates were influenced by price-formations rather than vice versa.

Apart from this, we may also appeal to the well-known statistical fact that price-changes almost in every case have preceded changes in the interest rates, as partial proof of the *passive* influence having been the stronger in the past than the *active* one. It may, of course, be contended that deliberate control of prices does in fact imply this sort of lag, but this need not necessarily be the case. What is there, *e.g.*, to prevent the banks from forestalling precipitate price-changes? The very fact that in the past control of price-movements has been imperfect to an extreme degree supports the explanation suggested above.

The second point that may be stressed in this connection is that the relation in question is not simply true of the long rate but of the market rate as the representative of the whole interest-rate mechanism. On this there need not be any difference of opinion.

But the question that remains to be answered is, whether it is necessary to postulate that the natural rate diverges from the market rate over longish periods. There seems to be no need to make this assumption. The phenomenon can well be explained keeping the Keynesian short-period $I = S$ theory intact. For, it may be convenient to regard the natural rate as itself being at a high level, when the trend of prices is a rising one and at a low level when it is a falling one, and to assume that the market rate, over longish periods, keeps company with the natural rate in its upward and downward movements, wherever equilibrium is asserted. It is clear that when prices are on the up-grade, the pull of the demand due to investment may be expected to exceed the steady rate of social saving, so that the position of the natural rate will, on the average, be very high and contrarily when the prices are on the down-grade. This need not involve any long-period disparity between the market rate and the natural rate, although in the short period the market rate may well be held to oscillate about its ideal position, *viz.*, the natural rate.

CHAPTER XVI

CONCLUSION

IN the foregoing chapters, I had little to offer by way of a contribution to the solution of current problems of monetary control. All the arguments advanced to explain the "causes" of and to suggest "remedies" for the economic malady of the Depression have by now become fairly well-worn. To repeat them here would indeed be an essay in boring. The immediate question, it appears to me, is not how to give a good starting kick to the world in its attempt to get out of the quagmire and into the field of fresh activity, but to maintain and advance the little recovery which has been made during the last two or three years. For, whatever be the causal forces—and these appear, in cases, to be mutually contradictory and, in others, just blunders of the right variety—we are again on the threshold of a trade revival which may easily be thwarted by any untoward circumstance or any unwise move on our part. The new tendency towards revival needs patient watching and careful nursing.

Out of evil cometh good. The good that has come out of the present Slump is the breakdown of the international standard, the dethronement of King Gold from his proud position. It was becoming abundantly clear that the gold standard could no longer stand the strain of a growing world productivity under circumstances in which the rules of the gold standard game were being disregarded, and nations vied with each other in the Midasian virtue of cupidity. Not only was this the only cause of its undoing. The gold standard was becoming more and more incompatible with a social system, in which welfare was safeguarded on national lines, in which race, colour, politics, culture and economic life were all commensurate with national boundaries. It set limitations upon national progress by its conditions of international management and control. The absence of a

Super-State which would achieve all the far-off dreams of philosophers, and, above all, make mankind internationally-minded and make available to all alike the fruits of scientific progress, rendered the gold standard a mere ludicry in the scheme of world affairs. The gold standard upset the costs-prices equilibria of the world's nations and, especially in the field of international trade, was wholly responsible for contracting the channels of commercial intercourse owing to its disastrous repercussions on international prices relative to domestic costs. Not only the present Depression but the whole history of trade cycles during the last sixty years or so is explainable by reference to this inability of the gold standard to align prices with costs.

What conclusions of policy, then, do we arrive at from the foregoing discussions, as to management of money with a view to industrial stability? In the first place, we must divest our minds of the notion that the natural or ideal rate provides us with a tool for abnormal times. For abnormal times, equilibrium theory has little to offer by way of technique; in such times, for a seriously disequilibrated system, full use will have to be made of all the weapons at the disposal of monetary and governmental authorities: bank-rate manipulations, open-market operations, direct and indirect long-term investments, a judicious public-works policy and active propaganda,—all these, and many more, measures will have to be taken in a determined effort to rehabilitate an unbalanced economic system.

In normal times, on the other hand, equilibrium theory offers us a helpful instrument in the $I = S$ theory and costs-prices equilibrium for the control of money towards the end of industrial stability. It is, of course, a difficult task for human ingenuity to operate the delicate monetary apparatus so as not to produce disharmonies and disequilibria; but, I believe, with the advance of statistical knowledge and practice and with the greater use of statistical data by Central Banks in their monetary operations, we shall become more and more capable of handling this apparatus with efficiency and care. There is a message of hope in this, if we will heed it. We may not perhaps be able to

counteract all the disturbances set up by the new structural and physical forces, but instead of sitting with folded hands, we can well adopt the manlier course of doing all that we can, and, through a well-organised and genuine endeavour, realise all the immense economic possibilities that the future holds in store for us.

THE END

INDEX OF AUTHORS

- Akerman, 20 n., 93 n., 104 n., 106 n.
 Angell, J. W., 20 n., 106 n.
- Blackett, Sir Basil, 54 n.
 Böhm-Bawerk, 35, 37, 38, 86, 94, 95
- Cannan, 28, 92 n.
 Cassel, Gustav, 3, 15-20, 36, 39, 49, 49 n., 75-80, 86, 90, 93 n.
 Clark, Colin, 67 n.
 Copland, D. B., 85 n.
- Fetter, 49 n.
 Fisher, Irving, 3, 6 n., 8-14, 50, 51, 54, 63, 119, 120
- Gibson, A. H., 6 n., 46, 119-22
- Hardy and Lyon, 43 n.
 Hawtrey, R. G., 44, 51
 Hayek, F., 3, 19, 20 n., 24, 28 n., 29, 30, 30 n., 32-40, 45, 46, 54 n., 56, 58 n., 86, 89, 90, 90 n., 104
- Jones, J. H., 7 n.
- Keynes, J. M., viii, 3, 4 n., 10 n., 12, 19, 23, 24, 30, 30 n., 36, 38, 41, 42, 44, 45-52, 53, 55 n., 56, 56 n., 57, 57 n., 62 n., 64 n., 67, 74, 75, 78, 93 n., 102, 103, 109 n., 112, 114-18, 119, 120, 121, 122
- Knight, F., 37 n., 104 n.
 Kock, Karin, 116
- Marshall, 50, 51, 54
 Meade, J. E., 58 n.
 Mitchell, W., 21, 222
 Myrdal, G., 29
- Ohlin, 79
- Peake, 119
 Pigou, 66 n., 100
- Ramsey, F. P., 62
 Ricardo, viii, 3, 24
 Richards, C. S., 85 n.
 Riefler, ix, 114-18
 Robbins, Lionel, 86 n.
 Robertson, D. H., ix, 24, 36, 40, 48 n., 49 n., 53, 53 n., 60, 93 n., 99, 103 n., 109
 Roscher, 87
- Simmons, ix, 114-18
 Smith, Adam, 3
 Snyder, Carl, 21
 Sraffa, P., 3, 12, 15, 24, 30, 30 n., 31 n., 41-4, 89 n.
- Taussig, 79
 Thomas, Brinley, ix
 Thornton, 3
- Viljoen, Stephen, 65 n.
 Viner, Jacob, 79
- Wicksell, 3, 6 n., 15, 15 n., 18, 19, 24-31, 49, 49 n., 55 n., 59

BOOKS TO READ

WORLD DISLOCATION AND WORLD RECOVERY : Agriculture as the Touchstone of Economic World Events.

By Dr. W. H. C. KNAPP.

Demy 8vo. 212 pp.

10s. 6d.

The committee of the International Agricultural Institute at Rome has awarded to this book, out of thirty-three competitors, the prize Humbert-Marie José as being the best work on economics of international importance.

SOME ASPECTS OF INDIAN FOREIGN TRADE, 1757-1893.

By IGNATIUS DURGA PARSHAD, M.A.

Demy 8vo. 256 pp.

12s.

This work traces the growth and development of Indian Foreign Trade under the influence of economic and political forces, both at home and abroad. A careful analysis of the causes which led to its growth and of those which restricted it, along with their reactions upon economic conditions in India, has been made.

Special emphasis has been laid throughout upon the changes in the character and direction of the trade—two most interesting aspects of the subject which have hitherto not received the attention they deserve. Statements made and conclusions arrived at are based upon and supported by original and essential evidence.

THE CO-OPERATIVE MOVEMENT IN INDIA.

By ELEANOR M. HOUGH, Ph.D., with an Introduction by the
late Rt. Hon. SIR HORACE PLUNKETT, K.C.V.O.

Demy 8vo. 368 pp.

15s.

his book presents an exhaustive analysis of Co-operation and its problems in India, and makes a welcome addition to recent Co-operative literature, for it presents a dispassionate examination of the movement without bias, communal or regional. It is in no sense of the term a "tourist production," but presents the result of the author's first-hand investigations, aided by personal discussions with eminent Co-operative workers and visits to typical Co-operative organizations.

THE INDUSTRIAL EFFICIENCY OF INDIA.

By RAJANI KANTA DAS, M.Sc., Ph.D.

Crown 8vo. 220 pp.

8s. 6d.

his book estimates the extent of India's industrial efficiency and analyses causes of and suggests remedies for inefficiency.

It is shown that India wastes two-thirds of her land, labour and capital resources, and is only one-third as efficient as other industrial countries. Among other things the author suggests the establishment of a National Board of Industrial Efficiency for conducting researches and helping in the achievement of efficiency.

P. S. KING & SON, LTD.

14 Great Smith Street, Westminster

BOOKS TO READ

WORLD DISLOCATION AND WORLD RECOVERY: Agriculture as the Touchstone of Economic World Events.

By Dr. W. H. C. KNAPP.

Demy 8vo. 212 pp.

10s. 6d.

The committee of the International Agricultural Institute at Rome has awarded to this book, out of thirty-three competitors, the prize Humbert-Marie José as being the best work on economics of international importance.

SOME ASPECTS OF INDIAN FOREIGN TRADE, 1757-1893.

By IGNATIUS DURGA PARSHAD, M.A.

Demy 8vo. 256 pp.

12s.

This work traces the growth and development of Indian Foreign Trade under the influence of economic and political forces, both at home and abroad. A careful analysis of the causes which led to its growth and of those which restricted it, along with their reactions upon economic conditions in India, has been made.

Special emphasis has been laid throughout upon the changes in the character and direction of the trade—two most interesting aspects of the subject which have hitherto not received the attention they deserve. Statements made and conclusions arrived at are based upon and supported by original and essential evidence.

THE CO-OPERATIVE MOVEMENT IN INDIA.

By ELEANOR M. HOUGH, Ph.D., with an Introduction by the late Rt. Hon. SIR HORACE PLUNKETT, K.C.V.O.

Demy 8vo. 368 pp.

15s.

his book presents an exhaustive analysis of Co-operation and its problems in India, and makes a welcome addition to recent Co-operative literature, for it presents a dispassionate examination of the movement without bias, communal or regional. It is in no sense of the term a "tourist production," but presents the result of the author's first-hand investigations, aided by personal discussions with eminent Co-operative workers and visits to typical Co-operative organizations.

THE INDUSTRIAL EFFICIENCY OF INDIA.

By RAJANI KANTA DAS, M.Sc., Ph.D.

Crown 8vo. 220 pp.

8s. 6d.

his book estimates the extent of India's industrial efficiency and analyses causes of and suggests remedies for inefficiency.

It is shown that India wastes two-thirds of her land, labour and capital resources, is only one-third as efficient as other industrial countries. Among other things the author suggests the establishment of a National Board of Industrial Efficiency for conducting researches and helping in the achievement of efficiency.

P. S. KING & SON, LTD.

14 Great Smith Street, Westminster

BOOKS TO READ

THE ECONOMICS OF CONSUMERS' CREDIT.

By FERDYNAND ZWEIG, Docent in the University of Cracow.

Crown 8vo. 118 pp.

5s.

In view of the intense discussion going on at present on the subject of reviving economic life by various schemes of financing consumption, this book of Dr. Zweig's is timely and will prove valuable to economists, financiers and politicians. The book is divided into eight parts: I. Theory of Consumers' Credit; II. Financing Consumption; III. Ideas underlying the Financing of Consumption; IV. Theoretical Justification; V. Effects on Trade; VI. Effects on Saving; VII. Applicability and Working; VIII. Conclusion.

MONETARY OPINIONS AND POLICY, 1924-34.

By MARY THERESA RANKIN, M.A., D.Phil.

Crown 8vo. 164 pp.

6s.

A series of addresses delivered to public bodies in Edinburgh on post-war monetary questions as they arose for consideration. The express purpose of the addresses was the informing and moulding of public opinion on matters of monetary policy. The series thus forms both a connected history of monetary events and problems of the period, and a critical exposition of the principles and theories involved, and in a form especially valuable to the non-academic economist and the man of affairs desirous of forming judgments on future policy—the more so that the general standpoint throughout is one that events have justified.

THE VALUE OF MONEY: A Discussion of Various Monetary Theories, and an Exposition of the Yield Theory of the Value of Money.

By TJARDUS GREIDANUS.

Demy 8vo. 372 pp.

15s.

This book gives a classification and discussion of a number of theories dealing with the problem of the value of money. These considerations are followed by the author's attempt at finding a solution which explains the value of money from its interaction with the yield that money is able to produce as an instrument of trade.

GOLD, UNEMPLOYMENT AND CAPITALISM: Essays and Addresses.

By T. E. GREGORY, D.Sc., Sir Ernest Cassel Professor of Economics in the University of London.

Demy 8vo. 320 pp.

12s.

"Gold, Unemployment and Capitalism" is a selection from essays in a wide range of economic subjects written, with one exception, during the last eight years. The main divisions of the book—Gold, America, Central Banking, International Trade and Unemployment and Capitalism—show clearly the subjects with which Professor Gregory deals, and the whole collection is an interesting survey of what has been happening in the economic field during these last few difficult years, and of a distinguished economist's reaction to these events.

P. S. KING & SON, LTD.

14 Great Smith Street, Westminster

BOOKS TO READ

THE ECONOMICS OF CONSUMERS' CREDIT.

By FERDYNAND ZWEIG, Docent in the University of Cracow.

Crown 8vo. 118 pp.

5s.

In view of the intense discussion going on at present on the subject of reviving economic life by various schemes of financing consumption, this book of Dr. Zweig's is timely and will prove valuable to economists, financiers and politicians. The book is divided into eight parts: I. Theory of Consumers' Credit; II. Financing Consumption; III. Ideas underlying the Financing of Consumption; IV. Theoretical Justification; V. Effects on Trade; VI. Effects on Saving; VII. Applicability and Working; VIII. Conclusion.

MONETARY OPINIONS AND POLICY, 1924-34.

By MARY THERESA RANKIN, M.A., D.Phil.

Crown 8vo. 164 pp.

6s.

A series of addresses delivered to public bodies in Edinburgh on post-war monetary questions as they arose for consideration. The express purpose of the addresses was the informing and moulding of public opinion on matters of monetary policy. The series thus forms both a connected history of monetary events and problems of the period, and a critical exposition of the principles and theories involved, and in a form especially valuable to the non-academic economist and the man of affairs desirous of forming judgments on future policy—the more so that a general standpoint throughout is one that events have justified.

THE VALUE OF MONEY: A Discussion of Various Monetary Theories, and an Exposition of the Yield Theory of the Value of Money.

By TJARDUS GREIDANUS.

Demy 8vo. 372 pp.

15s.

This book gives a classification and discussion of a number of theories dealing with the problem of the value of money. These considerations are followed by the author's attempt at finding a solution which explains the value of money from its interaction with the yield that money is able to produce as an instrument of trade.

GOLD, UNEMPLOYMENT AND CAPITALISM: Essays and Addresses.

By T. E. GREGORY, D.Sc., Sir Ernest Cassel Professor of Economics in the University of London.

Demy 8vo. 320 pp.

12s.

"Gold, Unemployment and Capitalism" is a selection from essays in a wide range of economic subjects written, with one exception, during the last eight years. The main divisions of the book—Gold, America, Central Banking, International Trade and Unemployment and Capitalism—show clearly the subjects with which Professor Gregory deals, and the whole collection is an interesting survey of what has been happening in the economic field during these last few difficult years, and of a distinguished economist's reaction to these events.

P. S. KING & SON, LTD.

14 Great Smith Street, Westminster

